Volume XVII. *October, 1903.* *Number 4.*

THE

PHYSICAL REVIEW

A DETERMINATION OF THE HEAT OF FUSION OF ICE.

By Arthur W. Smith.

Introduction.

At the present time there is no constant of nature of greater importance, and at the same time less well known, than the heat of fusion of ice. One naturally refers to the classic researches of Regnault, and to the later determination of Bunsen as fully determining this constant. But a comparison of the values obtained by these two investigators shows that they differ by 1 part in 100, and a more critical examination of the original memoirs is sufficient to convince one that neither determination is all that could be desired. Determinations made by other investigators, while agreeing well among themselves, give results which differ considerably from each other. At the present time it is safe to say that the heat of fusion of ice is known only to 1 part in 200. This wide variation was pointed out three years ago by Professor J. S. Ames[1] in his paper on the "Mechanical Equivalent of Heat," and at that time he suggested to the writer the need of a more exact determination of this constant. It was not until the present year, however, that it was possible to take up this investigation and carry it through.

The whole subject of calorimetry must be regarded as still in its infancy as long as its measurements are expressed in variable and unknown units. The usual unit employed, viz., the gram-degree

[1] Paris Reports, 1900, Vol. I., p. 178.

calorie, can never become a satisfactory standard for the measurement of quantities of heat inasmuch as it involves the measurement of changes in temperature of some substance and all the inherent uncertainties connected with such measurements. This is doubly true when the substance employed has a varying heat capacity, as is the case with water. A far more satisfactory unit would be one expressed in terms of the heat required to produce a change of state without any attendant change of temperature. The most convenient unit for practical purposes and fulfilling these conditions, is found in the heat of fusion of ice, and it is surprising that no determination of this very important constant has been attempted for a third of a century, and that no electrical method has ever been used.

Electrical measurements have now attained such a degree of perfection that the heat generated by an electric current can be determined with an accuracy far surpassing any direct calorimetric method. The problem which I have undertaken to solve is the determination of the heat of fusion of ice in terms of the electrical units.

PREVIOUS DETERMINATIONS.

The fact that when heat is given to a piece of ice its temperature is raised until 0° C. is reached, at which point it remains while large quantities of heat continue to be added, was first pointed out by Black [1] in his remarkably clear and explicit lectures on chemistry delivered in Edinburgh in 1762. The heat which was thus used in melting the ice without producing any change in temperature was called by him "latent heat," a name which, although not altogether felicitous, has clung to it ever since.

Black determined the amount of this "latent heat" by several methods. In one experiment he measured the time required for the conversion of a known quantity of ice at 32° F. into water at 40° F. in a room of which the temperature remained constantly at 47° F., and compared it with the time during which the temperature of an equal weight of water rose under similar circumstances from 33° F. to 40° F. He thus obtained for the heat of fusion of ice the number 139° F. In another experiment 119 parts of ice at

[1] Black, Lectures on Chemistry, Vol. I., pp. 120–127.

32° F. were melted in 135 parts of water at 190° F., giving 254 parts of water at 53° F. Hence taking into account the different specific heats of the water and the containing vessel, he deduced the number 143° F. In a third experiment, which is really a modification of the latter, the water was warmed to 176° F. and mixed with an equal weight of ice. The resulting mixture was water at 32° F., giving for the heat of fusion the number 144° F. The last two methods are much more accurate than the first, and give the mean value 143°.5 F. or 79°.7 C.

A few years later Wilke, a Swede, determined the heat of fusion of ice by a different method. He took two similar vessels, one filled with water at 0° C. and the other with an equal weight of snow at the same temperature, and placed them both in boiling water. When the thermometer in the first vessel reached 72° C. he quickly removed the second, assuming that it had received the same amount of heat as the first. The temperature indicated was + 2° C. and a bit of the snow was unmelted. This soon melted, reducing the temperature to 0° C., and therefore Wilke took the number 72 as representing the heat of fusion of the snow. The inherent errors of this method are so great the wonder is that they balance each other as nearly as they appear to do.

The first determination of the heat of fusion of ice worthy the name was made in 1780 by Laplace and Lavoisier.[1] In their work on specific heats they used an ice calorimeter consisting of an inner chamber surrounded on all sides by finely broken ice. This ice was protected from outside heat by another covering of ice. A warm body placed within the chamber would melt some of the surrounding ice, the resulting water being allowed to drain away to where it was collected and measured. Equal weights of different substances, when warmed to the same temperature and placed within the calorimeter, were found to melt different amounts of ice in cooling to 0° C. By comparing the amounts of ice thus melted the relative specific heats of different substances were obtained.

In order to express these results in terms of water it was necessary to determine the amount of ice which would be melted by a

[1] Lavoisier et Laplace, Memoires de l'Academie des sciences, 1780, p. 355; Oeuvres de Lavoisier, t. I., p. 283.

given amount of water in cooling one degree. A vessel of sheet iron, weighing with its cover 1.7347 pounds, was filled with water, 2.74349 pounds, and placed in boiling water. The temperature attained was 79°.5 R. The vessel with the hot water was transferred to the interior of the ice calorimeter, and at the end of sixteen hours it had reached the temperature of the ice. The amount of ice melted was 3.966797 pounds, of which 0.252219 pound was due to the sheet-iron vessel, determined by a separate experiment. The ice melted by the hot water was, then, 3.714578 pounds. (Doubtless these figures were obtained by calculation, and do not imply extreme accuracy.)

In another experiment the warm water was poured into the calorimeter and, apparently, directly upon the ice. The amount of water used was 4 lb. 8 oz. at 70° R. There was removed from the calorimeter 9 lb. 12 oz. of water at 0° R. Hence 5 lb. 4 oz. of ice was melted by the 4 lb. 8 oz. of water. In order to have melted an equal weight of ice the temperature of the water should have been 60° R. Another experiment gave the number 60°.856. The mean of these values, "and several others" not published, was taken in round numbers to be 60° R. This is equivalent to 75° C., and this value of the heat of fusion of ice was accepted and used by physicists for the following sixty years — from 1780 till 1840 or later.

The first determination of the heat of fusion of ice which bears the marks of accuracy is that of La Provostaye and Desains,[1] in 1843. These investigators used the method of mixtures, strictly so called. That is, pieces of ice from the surface of which most of the water had been removed with tissue paper, were dropped into water and the lowering of temperature caused by the melting of the ice was measured with a thermometer. Their thermometers were graduated to tenths of a degree and read to hundredths, and were compared with standards. The amounts of water and of ice employed were determined by weighing the calorimeter first empty, then with the water, and finally after the ice was melted. The initial temperature of the water varied between 18° C. and 30° C., for the different experiments, being such in each case that with the

[1] La Provostaye and Desains, Ann. de Chem. et Phys., t. 8, 1843, pp. 5–19.

amount of ice used in the experiment the final temperature would be nearly the same as that of the surroundings. By this means the radiation correction of the calorimeter was made as small as possible. The rate of cooling of the calorimeter for each degree difference in temperature between the water in the calorimeter and the surroundings was determined by a preliminary experiment. During an experiment the temperature was recorded every few seconds, and the observed final temperature was corrected for the cooling by radiation.

They estimated the amount of water clinging to the ice as about one two-thousandth of the weight of the ice, and as therefore too small to take into account in their experiments. The mean of seventeen determinations of the heat of fusion of ice gave the value 79.01 calories in terms of the mean specific heat of water over the range 10° C. to 24° C. The authors show that this value is subject to a probable error of 0.30 calorie.

The weights of the calorimeter with the water and the ice were corrected for the loss due to evaporation. The loss of heat due to this cause if taken into account at all may have been included, consciously or unconsciously, in the determination of the rate of cooling. But in the single detailed calculation given, if an amount of water as great as assumed, and by which the weights are corrected, actually did evaporate, the cooling produced thereby would be 160 per cent. of the total cooling correction applied.

The results obtained by La Provostaye and Desains were confirmed by the experiments of Regnault[1] made the same year. He also used the method of mixtures and pointed out the errors due to the water introduced on the ice, and to the evaporation of water from the calorimeter. No corrections were made, as these were considered too small to affect the result. This is true regarding the change in weight, as the maximum evaporation was only 0.07 gram, but the heat required to evaporate this water, if neglected, would make the computed value of the heat of fusion too great by one third of a unit.

During the winter of 1842 Regnault made several determinations of the heat of fusion of snow. The temperature of the snow was measured with a thermometer, and was always a few tenths of a

[1] Regnault, Ann. Chem. et Phys., 1843, t. 8, pp. 19–27.

degree below 0° C. When transferred to the calorimeter the snow melted very quickly, thus reducing the errors due to radiation. The specific heat of the snow was assumed to be the same as for water. The mean of four determinations gave the result 79.24 calories.

There being little snow in Paris the following winter, he used pieces of ice cut from a clear block and free from air bubbles. The ice was dried with filter paper and quickly transferred to the calorimeter, its mass being determined by weighing the calorimeter before and after the experiment. The mean of thirteen determinations was 79.06 calories, in terms of water over a mean range of from 11° C. to 22° C.

In 1848 Hess[1] conducted an elaborate series of experiments to determine the heat of fusion of ice. He avoided the errors introduced by the layer of water on melting ice by using ice several degrees below zero. The room in which he worked was kept cool, while the balance, ice, etc., were in a glass case outside one of the northern windows. The temperature of the ice was measured by a thermometer in the midst of broken pieces in a beaker. The usual method of mixtures was used, the cold ice being placed in the water and allowed to melt. The temperatures were measured to tenths of a degree, and the amounts of ice and water so proportioned that the final temperature was that of the room. Forty determinations were made, each involving the two unknown quantities, the specific heat of ice and its heat of fusion. Solving for these quantities gave 0.533 for the specific heat of ice, and for the heat of fusion the value 80.34 calories in terms of water over the mean range of from 7° C. to 19° C.

The last investigation having as its object the determination of the heat of fusion of ice was made in 1850 by Person.[2] He criticises the values found by Regnault and La Provostaye and Desains because they had followed the methods of previous investigators and used ice at 0° C. and, therefore, had not taken into account the extra heat which, according to his notion, ice absorbs before reaching 0° C.

[1] Hess, St. Petersburg Imp. Acad. Sci. Bull. de la Classe Physico-mathematique, IX., 1851, pp. 81–85.

[2] Person, Ann. Chem. et Phys., 1850, t. 30, p. 73.

Person used the method of mixtures, the temperature of the water being about + 16° C. and that of the ice from — 2° C. to — 21° C. The ice was a cylindrical block, 4 cm. in diameter and 12 cm. high. This was enclosed in a sheet-iron case and contained a thermometer to measure its temperature. The specific heat of the ice was determined by cooling it to — 21° C. in a freezing mixture, then plunging it, case and all, into the calorimeter, which contained a saline solution a few tenths of a degree below 0° C. After ten or twenty minutes the final temperatures were read. Two determinations gave 0.48 for the mean specific heat of ice from — 21° C. to — 2° C.

The experiments for the determination of the heat of fusion of ice were conducted in a similar manner. The block of ice was cooled in a freezing mixture to several degrees below 0° C. and then plunged into the calorimeter, which contained water at about 16° C. Using the above value for the specific heat of ice, and unity for the specific heat of water, he obtained the value 80.0 for what he was pleased to call the "total" heat of fusion, this being the mean of six experiments. From the recorded data it is impossible to say what the error of this result may be. The only corrections applied are for the water equivalent of the calorimeter, thermometer, etc., and a correction for radiation — the determination of which is not given.

In 1870, Bunsen[1] devised the ice calorimeter which bears his name. As with the ice calorimeter of Laplace and Lavoisier, Bunsen used his instrument for the study of specific heats, and in order to express the specific heats of the substances studied in terms of the mean specific heat of water two auxiliary experiments were made. In these experiments he used 0.3333 gram of water enclosed in a glass tube and warmed to the temperature of boiling water. The water and tube were then dropped into the ice calorimeter, and the change in the scale reading observed. In the second experiment the same water and tube were warmed to identically the same temperature, and produced almost the same change in the scale reading of the ice calorimeter. Evidently the two experiments were made in quick succession without a single variation, and the two

[1] Bunsen, Phil. Mag., Ser. 4, Vol. 41, 1871, p. 182.

results, 80.01 and 80.04, of necessity agree very closely. The mean, 80.02, is expressed in mean calories.

The results of these determinations are collected in Table I. It

TABLE I.

Summary of Previous Determinations.

Name.	Date.	Number of Experiments.	Temp. Range of Water.	Heat of Fusion.	
				Calories.	Joules.
Black.	1762	2	80–0	79.7	
Wilke.		1	72–0	72	
Laplace and Lavoisier.	1780	2	100–0	75	
Provostaye and Desains.	1843	17	24–10	79.1	331.5
Regnault.	1842	4	16–7	79.24	332.7
Regnault.	1843	13	22–11	79.06	330.8
Hess.	1848	40	19–7	80.34	337.1
Person.	1850	6	16–5	80.0	335.9
Bunsen.	1870	2	100–0	80.02	335.2

is difficult to determine the exact temperature of the water used in these experiments but the attempt has been made to express the results in terms of joules, by making use of Barnes' values of the mechanical equivalent of heat, and the values set down in the table can not be far from right.

In all of these determinations of the heat of fusion of ice practically a single method has prevailed, that of mixtures. The limitations of this method are at once evident. No knowledge of the condition of the ice when it enters the calorimeter is possible. Unless it is melting, and has been in that condition a very long time, its temperature is entirely unknown. Experiments show that a block of ice which has been for several hours in a warm room (20° C.) may still be considerably below 0° C. in the interior. If such a block is broken up and soon used, even the small pieces will be colder than 0° C., although melting on the exterior. Again when the ice has been melting long enough to insure its temperature being very close to 0° C. it is impossible to remove all of the water from the ice, and an unknown amount of water is thus carried into the calorimeter and credited up as ice. It is not necessary that this water should be solely that of the superficial layer covering the ice. Unless the ice has been formed from exceedingly pure water,

and is uniform throughout, melting will occur at interior places, the resulting water remaining in its own cavity. The amount of water which may thus exist throughout the ice is neither determined nor removed, but is counted as so much ice.

Moreover, this method involves the measurement of changes in temperature, which at best involves considerable uncertainty, especially when mercury thermometers are used ; and as the results are expressed in indefinite units, the whole subject has been left in a most unsatisfactory condition.

General Method.

The method employed in the experiments now to be described may be briefly stated as follows: The sample of ice, whose heat of fusion is to be determined, is broken into small pieces and cooled several degrees below 0° C. While at this temperature it is weighed and transferred to the calorimeter, which contains kerosene oil also two or three degrees below 0° C. In this state there can be no question that the ice is entirely free from water, either on the outside or interior. The calorimeter and contents are slowly warmed by a very small electric current until the temperature reaches the desired point for commencing an experiment, usually about — 1° C. A larger current is then applied for sufficient time to melt the ice and raise the resulting water to about + 0°.5 C. At this temperature (of equilibrium) the ice has certainly all been melted, and the heat generated by the current has been used in four ways: (1) In raising the temperature of the ice and the calorimeter from about — 1° C. to 0° C. ; (2) in melting the ice ; (3) in raising the temperature of the water and calorimeter from 0° C. to about + 0°.5 C., and (4) in supplying whatever heat is lost by the combined effects of radiation, conduction, convection, etc. Of these four quantities of heat the second is thirty or forty times as great as all the others combined, which are determined as corrections, and when subtracted from the total amount of heat generated by the current, gives the heat required to melt the ice. This amount of heat divided by the mass of the ice gives the heat of fusion per gram of ice.

In order to provide a medium for the transference of heat from

the wire carrying the current to the ice, and to enable the different parts of the calorimeter to attain an equilibrium temperature, a bath of refined kerosene known as "Pratt's astral oil" was used. This oil could be cooled to any desired temperature without losing its fluidity, and it had no action on the ice — two points which are only possessed by this fluid, as far as could be learned.

The amount of heat produced in the heating coil by the current was determined by measuring the difference of potential between the terminals of the coil, the current which flowed through it, and the time during which the current was flowing. The heat, in joules, is then given by the formula EIt.

Each experiment is naturally divided into three parts: (1) The determination of the heat capacity of the calorimeter, ice, oil, etc.; (2) the melting of the ice, and (3) the determination of the heat capacity of the calorimeter, water, oil, etc. These are discussed in order below.

Heat Capacity of the Calorimeter, Ice, etc.— The amount of heat required to raise the ice, oil, calorimeter, etc., from the initial temperature to 0° C., was determined from the rate of warming during the preliminary heating. The current used for this was one tenth as large as that employed in melting the ice, thus generating one hundredth as much heat per minute. This small amount was used in order to warm the ice slowly and without melting any of it. As 0° C. is approached it was more and more difficult to apply heat and not allow any portion of the oil, even close to the wire, to become warmer than 0° C., but with care there was never any trouble from this source when the temperature was as low as — 1° C.

The heat capacity of the entire apparatus, including the ice, was found as follows: The temperature was read every minute, while the ice and oil were constantly stirred, until the rate of warming due to the combined effects of radiation, conduction, convection, stirring, etc., was nearly constant for ten or fifteen minutes. Then the small current was passed for ten minutes, the stirring being continued at the same rate, while the temperature rose more rapidly. Following this another series of temperature readings was taken while the calorimeter continued to warm up by heat from without. The rate at which the calorimeter was receiving heat from without

while the current was flowing, by Newton's law, which is shown below to apply to this case, is the mean of the rates just before and just after (since the temperature, on the average, is a mean between the temperatures before and after). The total amount of heat received by the calorimeter and its contents causes a certain rise of the thermometer, and since the temperature at the beginning and the end is changing very slowly it is safe to assume that the thermometer indicates the temperature of each part of the calorimeter, including the small pieces of ice. The difference between this observed change in temperature and that which would have been produced during the same time by outside influences alone, gives the change in temperature due to the current. This establishes the relation between heat (expressed in joules) and the change in temperature produced by the addition of this amount of heat. On the assumption that the same amount of heat will produce the same temperature change at any temperature up to 0° C., it is a simple matter of proportion to find the number of joules required to raise the calorimeter and contents from the initial temperature to 0° C. That this assumption is fully warranted is shown by experiments given below.

Melting the Ice. — The first part of the experiment is followed directly by the second, which differs from it only in the quantity of heat employed and the consequently greater changes in temperature, together with the melting of the ice. The current is about two amperes and flows for twenty or thirty minutes, while the stirring continues uninterrupted at the same rate as before. Two observers are required during this portion of the experiment — one to continue the stirring and read the thermometer and keep the records, while the other attends to the regulation and measurement of the electrical energy.

In series with the heating coil was placed a standard half ohm coil which was specially designed for carrying currents as great as two amperes. The current through these coils was maintained constant at about two amperes by regulating it so that the fall of potential in the standard coil always just balanced the E.M.F. of one standard Weston cell. Each minute, or as often as possible, the difference of potential between the terminals of the heating coil

was measured with a potentiometer and standard cell. The time during which the current was flowing was measured with an Elgin watch with the aid of a reading glass. This measurement of both the current and the difference of potential makes the experiment more complicated and laborious than it would have been simply to maintain a constant current through the coil and calculate the heat from the formula RI^2t, where R denotes the resistance of the heating coil, or to maintain a constant E.M.F. and calculate the heat from the expression E^2t/R. In each of these cases a knowledge of the resistance R would be required, and this is unknown without a knowledge of the temperature of the wire when carrying the current. This temperature may be several degrees warmer than the surrounding bath and in any case is very difficult to determine. All of this uncertainty regarding the resistance of the heating coil is avoided by the method here adopted, and the increased labor is more than repaid in the increased confidence in the measurement of the electrical energy.

Heat Capacity of the Calorimeter, Water, etc. — After the ice is melted and the current stopped, the water, oil and calorimeter come to some equilibrium temperature a few tenths of a degree above 0° C. The amount of heat which has been expended in raising the temperature above 0° C. is found by another experiment in all respects similar to that performed below 0° C., and just described. The heat capacity will, of course, be greater than before, since the calorimeter now contains water instead of ice. It has been shown by Barnes [1] that the specific heat of water exhibits no peculiarity in the neighborhood of 0° C., and therefore within the limits of accuracy here required, the heat capacity of the entire apparatus is the same at about + 0°.6 C. that it is over the range from 0° C. to + 0°.5 C.

Correction for Radiation, Conduction, Convection and Stirring. — There remains another important correction, viz., the heat lost to the calorimeter by radiation, conduction, convection, stirring, etc., during the period in which the ice is being melted. This is determined in much the same way as in the auxiliary experiments just described. The bulb of the thermometer is near the wall of the calorimeter and

[1] Barnes, PHYSICAL REVIEW, Aug., 1902.

it is assumed that the temperature of one represents the temperature of the other. Newton's law of cooling is assumed, that is, that the heat lost each minute is proportional to the difference in temperature between the wall of the calorimeter and the surroundings. From the data already obtained the rate of cooling (or warming) at several temperatures both above and below 0° C. is given. These rates of cooling are expressed in terms of the equivalent joules per minute and plotted as ordinates against the corresponding temperatures as abscissæ. The curve joining all these points is very nearly a straight line, and if Newton's law is true it should be absolutely a straight line. Therefore the straight line which lies nearest to the plotted points is drawn, and this line then gives the heat lost per minute at any temperature.

The heat lost by the calorimeter through the combined effects of radiation, conduction, convection, stirring, etc., during the interval in which the ice is melting is then obtained from this curve as follows. As the temperature is measured each minute, the average of these readings gives the mean temperature during the experiment. The ordinate on the curve corresponding to this temperature gives the mean loss of heat per minute This quantity multiplied by the time in minutes gives the total loss of heat by radiation, etc.

Description of Apparatus.

In devising a form of apparatus suitable for the accurate determination of the heat of fusion of ice, two things were especially desired. First, that it would admit using different samples of ice, and ice formed under different circumstances, and secondly, that it should have as small a radiation constant as possible. The first condition debarred the Bunsen calorimeter, or any similar instrument, even if it had not been shown [1] that the density of ice depended upon the method of manufacture, and therefore rendered this instrument incapable of accurate results.

After a number of preliminary experiments upon the radiation constants of different calorimeters (*i. e.*, the heat lost or gained per minute per degree difference of temperature between the calorimeter and its surroundings) it was found that the best protection

[1] Nichols, Phys. Rev., Vol. 8, pp. 21–37, 1899.

against radiation was the double wall of a Dewar bulb. Accordingly a half-liter Dewar bulb was used for the basis of the calorimeter. This was supported within the chamber *D*, Fig. 2, by a special wooden clamp which fitted around the neck of the bulb. The upper portion of this clamp passed upwards through the tube *T*, which it completely filled and by which it was supported. The thermometer, stirrer rod, and current leads passed through appropriate holes, drilled lengthwise in this clamp support.

This form of calorimeter proved very satisfactory and fulfilled the highest expectations regarding the loss or gain of heat by conduction and radiation. When filled with water a few degrees below the surroundings it warmed up at the rate of 0°.0016 C. per minute per 1° C. difference in temperature between the interior and the exterior.

A preliminary experiment to see how rapidly ice could be melted without unduly raising the temperature, was performed as follows: Since only qualitative results were desired the bulb was filled with water instead of kerosene, and about a hundred grams of broken ice dropped in. On supplying heat by a current through the coil, the gratifying result was obtained that 50 watts raised the temperature to only + 0°.5 C. In other words, ten grams of ice could be melted per minute without raising the temperature over 0°.5 C. and consequently without producing much loss of heat by radiation.

But this fair beginning was doomed to speedy disappointment. In the midst of the next experiment, the inner wall of the bulb exploded with a loud report, the contents and broken glass being caught and held by the outer wall, which did not break. No possible reason could be found why the bulb should have given away at just that time. Fortunately the thermometer, which was within the bulb, escaped uninjured. Several weeks were spent in trying to obtain a duplicate bulb. The first one obtained had a much smaller neck, and while superior to the original in some respects it could not be used on the same supports, and did not give sufficient space for the thermometer, stirrer, current and potential leads, etc. After much correspondence, a bulb was obtained very like the original, and the experiments were resumed.

On trying the complete experiment and using kerosene as the

fluid, the ice, as expected, sank to the bottom of the bulb. When the current was passed to melt the ice, the temperature was found to be much higher than it was in the preliminary experiments in which the ice floated at the top of the warm fluid (water). This was doubtless due to the warmer oil rising to the top and away from the ice, which remained in a more or less compact mass at the bottom and presenting a minimum of surface to the action of the oil.

Many ineffectual attempts were made to support the ice in the oil with the heating coil below it. Various kinds of stirrers were tried in hopes of securing a more thorough circulation of the oil amongst the pieces of ice, but with limited success. The principal difficulty was in the smallness of any arrangement which could be passed through the neck of the flask. Finally, almost by accident, a form of rotary stirrer was found which secured complete circulation and promised satisfactory results. But disappointment was again in store. One day as the bulb containing some oil was resting upon the bed of excelsior where it was kept when not in use, a loud report was heard, and that bulb was no more. The destruction was complete, some of the pieces being thrown a distance of a meter away. I was only thankful it did not occur when in use, with the delicate thermometer inside.

Of course another bulb could have been obtained, but the danger of its exploding at any moment, and especially the risk of destroying the thermometer, was so great, it was decided to use some more stable form of apparatus. Moreover it was now the first of January with cold weather approaching, during which all the experiments would have to be performed. The shortness of time forbade much waiting for the construction of elaborate apparatus, and, therefore, the best that lay at hand was tried. While not equal to the Dewar bulb in preventing loss of heat by radiation, still it was very good, and the certainty of its remaining intact fully balanced any inferiority in other respects.

Final Form of Calorimeter. — The calorimeter as finally used is shown in section in Fig. 1, which is nearly self explanatory. *A* is a half-liter brass vessel, nickel plated and highly polished on the outside. Within this is shown the stirrer and the heating coil. *cc* is a brass tube over which is wound five ohms of No. 22, silk cov-

ered, manganin wire. The wire is insulated from the tube by a double thickness of silk, and the whole covered with several coats of shellac well baked on. The ends of the coil are soldered to two heavy copper leads, *aa*, to the upper ends of which are soldered the current and potential leads. The stirring is effected by a disk of brass, *dd*, which nearly fits the calorimeter, and is soldered to the lower end of the brass tube, *cc*. It is perforated by a number of small holes through which some of the oil can circulate while the greater part of it streams through the heating tube as the stirrer is moved up and down. The stirrer rod, *rr*, is a hard-rubber tube which screws onto the end of one of the heavy copper leads. It extended upward to the outside alongside the thermometer, Th, and also served to support the current and potential wires.

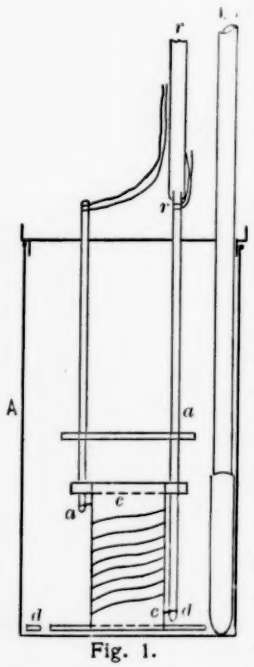


Fig. 1.

On one or two occasions during the preliminary experiments a few drops of oil were found to have spilled out of the calorimeter, which of course would seriously affect the weight of the apparatus at the end of an experiment, to say nothing of the change in the heat capacity. To prevent this as far as possible a tightly fitting cover was made. Openings were necessary for the thermometer, the stirrer rods, and for the tube through which the ice was dropped. A lid was arranged to fall and cover the latter opening when the tube was withdrawn. An edge was placed around the cover to hold whatever oil might find its way to the top. This precaution may have been unnecessary, as no oil was ever found on the cover.

The calorimeter was supported in the following manner. Sufficient cotton wool was wrapped around it to just slip into a large glass battery jar. This jar rested upon a wooden block at the bottom of the chamber *D*, Fig. 2, and to prevent as far as possible the loss of heat by convection air currents the remaining space was filled with cotton wool.

The dimensions of this chamber are 35 cm. in depth by 23 cm. diameter, it being circular in section. The larger vessel, *E*, is 70 cms. deep and 45 cm. in diameter, it also being circular in section. Both of these vessels were made of heavy galvanized iron. The space between them was filled with broken ice, making a layer 11 cm. in thickness around the sides of the inner chamber, and about twice this thickness over the top and bottom, thus maintaining the temperature of the interior chamber very closely to 0° C. The top of this chamber is a removable cover, made with an outside flange to prevent water from gaining access to the inside. At the center of this cover is a tube 4 cm. in diameter and 20 cm. in length, which extended through the broken ice to the outside, and through which passed the thermometer, stirrer rod, current wires, etc. In order to protect this broken ice as much as posr sible from outside heat, the vessel *E* was placed within an extra large barrel with the intervening space packed with excelsior. With this protection the amount of ice melted was about ten kilos per day, this amount being added each morning. The total quantity of ice required for this packing around the chamber was about sixty kilos. As fast as it melted the water was drained away through the tube *P*. Over the barrel was a wooden cover through which projected the tube *T*, the wooden post, *W*, which served as a support for the thermometer when in use, and the thermometer case, *L*, in which the thermometer was kept during the time between experiments.

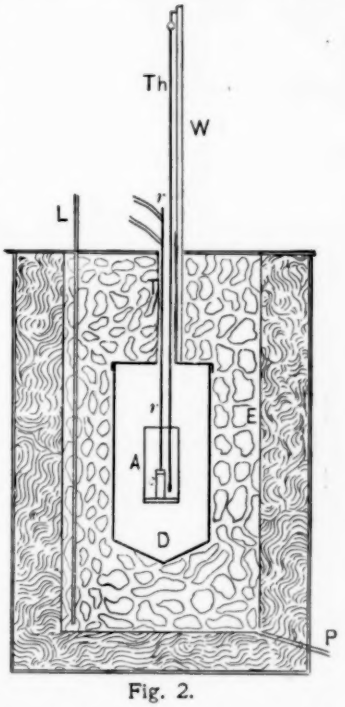


Fig. 2.

In order to fill the calorimeter with oil and ice a thin brass tube was arranged parallel to the thermometer and extending from the outside to about half a centimeter below the cover of the calorimeter. Within this tube was a glass funnel through which the cold

oil was poured. The funnel was then removed and the ice dropped through the tube, after which the tube was removed, the small trap door falling and closing the opening in the cover. The tube T was then filled with cotton to prevent circulation of the air between the inner chamber and the outside, it being firmly packed around the thermometer and stirrer rod, but allowing the necessary motion of the latter.

Electrical Arrangements. — A diagram of the electrical connections is shown in Fig. 3. C is the heating coil within the calorimeter, M is the standard half-ohm coil, while D is an auxiliary coil of resistance equal to C and M together. The current was obtained from eight storage cells, B, and by means of the switch S, could be passed through the auxiliary coil D while it was adjusted to approximately the proper value by the variable resistance R. The latter consisted of a rheostat containing a total of six ohms of heavy iron wire, in parallel with which was a liquid resistance of copper sulphate solution. By this means as small variations in the current as desired could easily be made. When ready to use the current in the calorimeter, the double pole, double throw switch S was closed on the other side, and the current quickly brought to the proper value by a very slight adjustment of the resistance R.

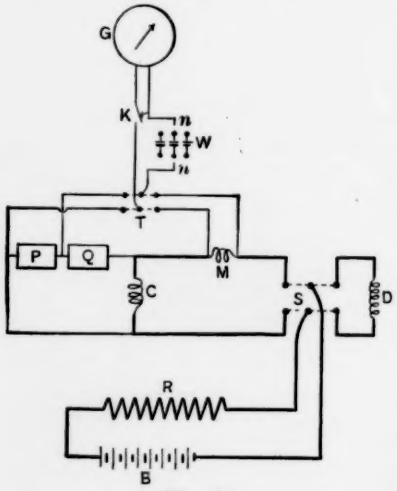


Fig. 3.

The galvanometer, G, was the Rowland D'Arsonval wall type and quite sensitive. Its free period of vibration was about 15 seconds, with little damping and requiring several minutes to come to rest after a deflection. In this work it is necessary to take readings almost continuously, which requires a dead beat instrument, and the galvanometer was modified in the following manner. A large mica vane was fastened to the back of the mirror; a copper tube was fitted over the coil so as to turn with it through the magnetic field

between the poles of the permanent magnet; and the coil of the galvanometer was short-circuited by the key *K* when open. These three methods of damping rendered the galvanometer sufficiently dead beat to be used, if care was taken never to allow deflections of more than 20 mm. Usually, however, the deflections were about 1 or 2 mm.

Three Weston standard cells, *W*, were used for the measurement of the current and E.M.F. They were connected to three pairs of mercury cups, as shown, and any one of them could be inserted in one of the galvanometer leads by placing the wires, *nn*, into the corresponding mercury cups. The ends of these wires were fastened in a block of hard rubber and projected about a centimeter beneath. By simply lifting this block from one pair of cups to another any one of the standard cells could be brought into use. One cell was used throughout one experiment (about half an hour), during which interval its constancy was several times tested by substituting an unused cell in its place for a single reading. No sign was ever found that the cells suffered the least change in their E.M.F. when in use. These cells were certified to have a constant E.M.F. at all temperatures between 10° C. and 35° C., but they were always used at very nearly 20° C.

The absolute value of the E.M.F. of these cells was determined by the National Bureau of Standards, where one of these cells (No. 329) was compared with ten standard Clark cells. Assuming the Clark to be 1.434 volts at 15° C. the E.M.F. of this cell was found to be 1.0193 volts at 18° C.

The results of all the experiments, save two, are expressed in terms of this cell. In the other two the standard cell No. 328 was used, the E.M.F. of which was less than the other by 1 part in 10,000, as shown by the maker's certificates, and also by a direct comparison at the time they were used. Hence the E.M.F. of this cell is taken as 1.0192 volts.

The standard half-ohm coil, *M*, is of the Reichsanstalt form as made by Leeds & Co., and was made with special reference to carrying currents as great as two amperes. It is provided with both current and potential terminals, the half ohm being the resistance between the two points where the potential terminals are attached.

The exact value of this resistance was determined by the National Bureau of Standards and was found to be 0.50008 ohm at 20° C. in terms of the mean values of two coils which were redetermined at the Reichsanstalt in July, 1902.

As shown in the figure, the current terminals are joined in series with the heating coil, and the potential terminals are joined to one side of the double pole, double throw switch, *T*. When this switch is closed the difference of potential between the terminals of the standard half ohm is balanced against the E.M.F. of whichever standard cell is in circuit, the difference, if any, being indicated by the galvanometer on closing the key *K*. The main current through *C* and *M* is continually adjusted to keep the galvanometer deflection zero. The observer sat with his eye at the telescope watching the galvanometer deflections, while one hand continually tapped the key *K*, and the other varied the resistance *R* by whatever amount was necessary to maintain zero deflection.

In order to be protected as far as possible from fluctuations in temperature, the standard cells and resistance were placed in a wooden box and covered with cotton wool. This box occupied a position on the table, found by trial, such that the heat from the register and the cold from the window balanced to give a nearly constant temperature of about 20° C. Quite large fluctuations in the temperature of the room, if not continued too long, produced very little change within the box.

Measurement of Current. — As explained above, the current through the heating coil was maintained constant by keeping the galvanometer deflection as near zero as possible. The deflections to one side or the other were usually less than 1 or 2 mm., and the corresponding steady deflections, had they been observed, would have been considerably less. As the deflections were as often one way as the other, the too small current at one time would tend to balance the too large current of another, thus making the actual average current through the calorimeter differ less from its intended value than if the deflections were all in the same direction. The sensitiveness of the entire arrangement was such that a steady deflection of 1 mm. corresponded to a variation in the current of about 1 part in 10,000.

Knowing the resistance of the standard coil M, and the difference of potential between its terminals, the current it carries is readily calculated by Ohm's law. The current in the heating coil C is equal to that in M, less the small portion which flows through the potentiometer circuit PQ.

Measurement of E.M.F. — The difference of potential between the terminals of the heating coil was measured by the usual potentiometer method as shown in Fig. 3. Q is a fixed resistance of 10,000 ohms, while P is about 1,100 ohms. Both resistances in series are joined in parallel with the heating coil and thus are subjected to a total fall of potential of about ten volts, of which a little over one volt is in the part P. As shown in the diagram, two lead wires from the terminals of P run to the switch T. On throwing the switch to this side, the fall of potential in P is compared against the E.M.F. of the same standard cell used for the measurement of current. The resistance of P is varied to give zero deflection of the galvanometer. Then, if E is the E.M.F. of the standard cell and P is the resistance corresponding to zero deflection of the galvanometer, the difference of potential, V, measured by the potentiometer is given by the expression

$$V = E\frac{Q+P}{P}.$$

When running an experiment, the switch T was kept closed on the current side in order to see that the latter remained constant. When everything was going smoothly, the switch was quickly thrown to the other side — the direction of the galvanometer deflection noted — and then thrown back again. The smallest change in P corresponded to a deflection of eight scale divisions, and when it was not possible to adjust P to give zero deflection, the small deflection was observed and the value of P corrected accordingly.

The temperature of the potentiometer coils, or at least that outside of the boxes which contained them, was about 20° C. in all of the experiments here reported. As it is the ratio of the resistances of these coils that is used in the measurement of E.M.F., any small change in temperature would affect this ratio very much less than it would the absolute value of the resistances. In fact it would be proportional to the difference between the temperature coefficients

of the two portions of the total resistance and is therefore inappreciable for a variation of one or two degrees, or even more.

Measurement of Time. — This was measured by an Elgin watch which was losing less than thirty seconds a month. The seconds hand was viewed with a small reading glass which magnified five diameters, and just as the 30-seconds mark was covered by the hand, the switch S was closed, thus starting the current through the coil C at the beginning of the 31st second. When it was desired to stop the current the same procedure was repeated. Whenever the length of time was arbitrary and would allow it, the current was allowed to flow an integral number of minutes, so that the seconds hand of the watch could be read in the same position at both the starting and the stopping of the current.

In order to obtain some idea of the accuracy with which intervals of time could be measured by this method, twelve single minute intervals as observed with the watch were recorded upon a chronograph sheet which was also receiving the records of seconds from the astronomical clock.

The duration of these intervals as obtained from the chronograph are given below.

60.22	60.15	60.15
60.23	60.22	60.25
60.15	60.15	60.10
60.20	60.15	60.15

Mean of all = 60.177, prob. error = 0.010
(One solar minute = 60.165 sidereal seconds).

The calculated probable error of one observation is 0.03 sec. and the greatest actual difference of a single observation from the mean is 0.08 second. From this it appears that intervals of time measured with the watch can be depended upon to at least a tenth of a second, and they probably possess a much greater accuracy.

The Thermometer. — The thermometer used in these experiments was one made to order by H. J. Green, of Brooklyn, N. Y. It was graduated on the stem to hundredths of degrees from $-13°$ C. to $+1°.6$ C. Its entire length was 95 cm. and it was 6 mm. in diameter. The bulb was long (8 cm.) and thin, thus quickly taking the temperature of the bath in which it was placed. The stem from $-5°$ C. up was above the top of the barrel, and with the

aid of a small reading glass the temperature could easily be read to thousandths of a degree.

The zero of this thermometer was too low by 0°.035, that is, the temperature of melting ice gave the reading + 0°.035 C. This difference is doubtless due in part to the fact that the stem was exposed to the temperature of the room (about 20° C.), but as this was the same in the actual experiments and when the "zero point" was determined no error is introduced by not applying the stem correction. The "zero point" was determined before and during the experiments by three different methods, viz., by placing the bulb and several centimeters of the stem into a mixture of clear ice and pure distilled water; by plunging it into freshly fallen snow which was saturated with pure distilled water; and by placing it in pure distilled water which was being frozen in a large test-tube. Each of these methods gave the reading + 0°.035 C. as the true zero, and all readings of the thermometer are corrected by this amount.

The brass case of the thermometer was kept standing in the ice surrounding the chamber *D*, and when not in use the thermometer was always within this case and at a temperature not higher than two or three tenths of a degree above zero. During the entire series of experiments here reported the thermometer was not allowed to rise above + 2° C. and the lowest temperatures to which it was subjected rarely exceeded − 5° C.

Manipulation of the Ice. — The ice used in these experiments was very carefully selected, only that which was perfectly clear and transparent and free from bubbles being taken. The structure of all the ice used, both the commercial ice and that made from pure distilled water, was decidedly crystalline, the axis of the crystals being normal to the surface at which the ice was formed. A piece of ice which externally appeared the same in all directions quickly revealed its crystalline nature when the attempt was made to split it. If the edge of a sharp knife was pressed againt the ice to divide it along the axis of crystallization, it readily separated along an almost perfect plane. The same is true of the planes at right angles to the axis, provided it is not too near the end of the crystals. In any other direction, and even that perpendicular to the axis if too near the end of the crystals, the pressure of the knife resulted in

merely shattering the ice as though it were a bundle of glass tubes. By cleaving the ice in planes at right angles and parallel to the axis of crystallization it is possible, however, to divide it into small cubes with smooth surfaces, each one of which was as clear as the original piece.

As the weather was seldom cold enough to keep the ice from melting, about a hundred grams of these pieces were dropped into a beaker surrounded by a freezing mixture. To prevent their freezing to the sides of the beaker a few pieces were introduced at a time and constantly stirred until they were cooled below 0° C. and whatever free water there might have been on the surface was completely frozen. The beaker was then closed with a ball of cotton wool to prevent the warmer air from coming in contact with the ice, and left in the freezing mixture some minutes longer.

In the meanwhile the proper amount of oil was measured out into a flask, weighed and placed in a freezing mixture, where it was cooled to about — 6° C. This flask was now removed, and the oil quickly poured into the calorimeter through the special funnel. Both the flask and the funnel were then laid aside to assume the temperature of the room, when they were reweighed to determine just how much oil entered the calorimeter.

In order to weigh the ice without its melting the beaker containing it is removed from the freezing mixture, quickly wiped dry, wrapped with cotton and placed within a larger beaker, the whole being covered with a papier mâché cap. This arrangement was placed on the balance and weighed. It is then carried to the calorimeter, the cover and ball of cotton removed, and the ice poured through the brass tube into the cold oil below. The ice is directed into this tube by a funnel formed of filter paper. At no time was the appearance of the ice such as to indicate that any portion was melting, nor was any water ever caught by the filter paper as the ice passed over it. The fall of the ice was broken by striking the stirrer and thus splashing of the oil was prevented. Usually the ice was in the oil of the calorimeter within three minutes after it was taken from the freezing mixture, and thus the opportunity for any melting of the ice, or of any change in weight due to evaporation, was very slight indeed. After pouring the ice into the calo-

rimeter the beaker was closed with the cotton ball, the cover replaced and the whole immediately weighed, thus determining the amount of ice employed.

If the equilibrium temperature of the oil, ice and calorimeter was much below — 2° C. or — 2°.5 C., it was cautiously raised to this point and then left for an hour, during which time it slowly rose some tenths of a degree further. At this point readings of temperature were commenced, while the oil and ice were constantly stirred at a uniform rate. By alternate periods of warming due to radiation alone with periods in which the rate of warming is increased by an electric current through the coil, the relation between change of temperature and joules was obtained, as already explained.

When the temperature reached — 1° C., or thereabouts, the larger current was applied for a sufficient time (as determined by previous calculation) to melt the ice. During this time one observer constantly watched the galvanometer and maintained the current constant, while the other continued the stirring and read the thermometer each minute. These readings were utilized in the calculation of the heat lost by radiation during the experiment.

After the experiment was concluded the calorimeter with the oil and water was again weighed to furnish a check upon the weight of ice used. This weight was always within a few centigrams of the sum of the weights of the separate portions.

Sources of Error.

The various sources of error in this method are:

1. Heat lost by radiation, conduction, convection, etc.
2. Heat produced by stirring.
3. Electrolysis of the water.
4. Loss of heat by evaporation.
5. Use of the thermometer.

Under the first head it must be noticed that the temperature of the calorimeter was always within 2° of 0° C., while the temperature of the surrounding walls of the chamber were very close to 0° C. It is assumed, according to Newton's law, that the heat lost by radiation is directly proportional to the differenee between the temperature of the calorimeter and that of the surrounding walls. To

prove that the loss of heat did thus depend upon the temperature of the calorimeter at the time, and not upon previous temperatures, two determinations of the heat lost at a given temperature were made. In one this temperature was reached by adding some very cold oil, after which the rate of cooling was observed for a considerable time. Again this temperature was reached by quickly warming the oil several degrees by the electric current. The rate of cooling, after the first few minutes, was identical with the former, thus showing that it depended solely upon the temperature at the moment.

The loss of heat by convection air currents is reduced to a minimum by filling the space around the calorimeter with cotton wool.

Whatever heat may be conducted to the calorimeter from the outside by the wires conveying the current is included in the determination of the heat lost by radiation, etc. However this is very small, since these four wires are of No. 16 copper wire, and the heat would have to be conducted a distance of 30 cm. and through air at 0° C. The heat generated in these wires by the current is about one joule during each experiment and is, therefore, inappreciable.

It thus appears that the loss or gain of heat due to these causes is a definite quantity capable of being determined. As this determination is based upon measurements made just before and just after the main experiment, the total error introduced cannot exceed a few joules.

Heat Produced by Stirring. — By extremely vigorous stirring it was possible to produce enough heat to cause an observable effect upon the rate of cooling due to radiation, etc. As ordinarily used, however, the stirrer was raised and lowered about 3 cm. fifty or sixty times a minute. At this rate no different effect could be observed from that when the stirring was only four times per minute. The heating must, therefore, be very small, and whatever it is is taken account of in the correction which is determined for radiation, etc.

Electrolysis of the Water. — This is impossible at the commencement of an experiment as there is no water present. It can hardly

be present later as the coil is well coated with shellac, wetted with kerosene oil, and only a small portion of one end is ever below the surface of the water.

Loss of Heat by Evaporation. — In the usual method of mixtures there is always a considerable amount of evaporation from the surface of the water, and a consequent loss of heat. In these experiments, however, the water is covered by the oil and there is no evaporation. The oil itself evaporates exceedingly slowly, and whatever heat is thus used is included in the determination of the loss by radiation, etc.

Use of the Thermometer. — The thermometer was used to measure the initial and final temperatures for determining the amounts of the corrections to be applied. As this is a very good standard thermometer recently made by Green, it is very improbable that there is any appreciable error in the length of that portion of the scale here used. The greatest error is introduced in the assumption that the temperature indicated by the thermometer truly represents the temperature of all portions of the calorimeter. As these readings were taken only when the temperature was changing very slowly and while the contents of the calorimeter were thoroughly stirred, this assumption can not be very far from the truth.

Weighting of the Experiments.

Each of the eight experiments here reported was conducted with the greatest care, and there is no reason for discrediting the result of any one of them. Nevertheless there are certain circumstances to be noted which warrant greater confidence in some experiments than in others, and these considerations are taken into account by the following system of weighting.

The maximum weight given to any experiment is 6, made up as follows :

One point if there has been a direct determination of heat capacity with ice preceding the regular experiment of melting the ice.

One point if the regular experiment has been followed by a direct determination of heat capacity with water.

Two points for a short duration of the experiment, that is, less than forty minutes.

One point if all the ice is melted with a single application of the current.

One point if the rate of cooling at the end of the experiment is definite and constant.

PRELIMINARY EXPERIMENTS.

The first experiments were made using the clearest portions of the artificial ice furnished to the laboratory. It was intended to use this ice for the preliminary work and later to try some ice frozen from the purest water obtainable. It was expected from the statements of some of the earlier investigators along this line, especially the paper by Person, that the specific heat of ice would increase gradually with increase of temperature from its value at $-2°$ C., and lower temperatures, and possibly approach the value for water as 0° C. was reached. In determining the heat capacity of the combined apparatus with ice for temperatures approaching 0° C., this idea seemed to be corroborated by experiment. A great many experiments were made to determine the exact value of this increased heat capacity. At first all determinations were for temperatures below $-0°.5$ C., and the increased heat capacity of the ice did not exceed that of water. Later, when the range from $-0°.5$ C. to 0° C. was studied the heat capacity came out as being inversely proportional to the temperature below 0° C., increasing without limit as 0° C. was approached! This at once raised suspicions that something was wrong, and further investigation showed that there had been some melting of the ice. When the temperature of the oil and ice was already very near to 0° C. it is probable that the current, though very small, raised the temperature of some portions of the oil to such a temperature that local melting of the ice was possible. Whatever water was detached from the ice and remained in the oil in small drops, would not freeze again when the equilibrium temperature was reached. However it was never possible to find sufficient water to account for *all* of the heat absorbed, and doubtless the remainder was used in internal melting. As shown below, unless the ice is absolutely pure throughout, those portions containing the greatest amount of impurities will melt first and at the lowest temperatures. As the temperature is raised a

greater and greater amount of the impure ice will reach its melting point, requiring an ever-increasing amount of heat to produce a given change in temperature. The water from this melting could remain on the surface of the ice, or if the melting was internal it would remain in its own cavity. Although in contact with the ice this water (or rather solution) could not be expected to freeze again and return its heat of fusion to the oil, even though the equilibrium temperature was two or three tenths of a degree below 0° C.

Moreover, at this time the entire question was complicated by the possibility that the oil might have some action upon the ice. This hardly seemed probable from the nature of the substances, and the fact that other investigators have observed no action upon ice when placed in refined kerosene. It is proved below that there is no such action.

Specific Heat of Ice. — There was now an imperative demand that the specific heat of ice be carefully studied over the range of temperature from — 1° C. to 0° C. Ordinary distilled water was placed in a large test-tube with a smaller test-tube suspended in the center. The water was frozen from the bottom up, thus forming a cylinder of ice between the two test-tubes, about 7 mm. in thickness and as many centimeters in height. A new heating coil was arranged at the bottom of the calorimeter and occupying only about 1 cm. in depth. Over this was placed the test-tube containing the ice. Any possible action of the oil upon the ice was now entirely eliminated as they were separated by the glass wall of the test-tube. Heat was slowly applied by the small current through the coil, and the heat capacity of the ice determined as before. Although much less than before, yet at — 0°.2 C., it appeared as great as for water and at — 0°.1 C. it was twice as great. On removing the tube and closely examining it, a bit of water was seen at the point where the last ice was formed. Doubtless this drop contained the greater part of whatever impurities existed in the tube of water and being the last to freeze it was the first to melt.

After this only the purest water was used for making ice. Such water was obtained from Dr. H. C. Jones, who kindly furnished all that was needed. This water was prepared by distilling ordinary distilled water from a solution of chromic acid, the vapor being

then passed directly into a boiling solution of barium hydroxide to remove all traces of carbonic acid. The vapor was then condensed in a block tin condenser and collected in a glass bottle. The latter was previously treated with boiling hydrochloric acid for a long time in order to render the glass insoluble in water. This water was being continually distilled for use in conductivity experiments in the Physical Chemical Laboratory, and possessed a specific resistance of one megohm.

A portion of this water was thoroughly boiled to remove the dissolved air and the test-tube filled to the desired height. The inner tube was not used, but a layer of ice was frozen on the sides of the tube to about the same thickness as before, the residual water in the center being thrown out. The ice thus formed is very pure, whatever impurities there might have been in the water at the beginning being thrown out in the residual water. Two such tubes were prepared and placed in the calorimeter, which was previously filled with cold oil.

Observations were made for the determination of heat capacity, the same as in a regular experiment and already described. The same experiment was repeated the following day (Feb. 20). The effect of having the oil in contact with the ice was determined by filling the cavity in the center of the ice tubes with oil, and again taking a series of observations for heat capacity. Any melting of the ice by the oil would be manifest by the increased amount of heat required to produce a given rise in temperature.

The results of these determinations are summarized in the following table. The second column gives the temperature at which the determination was made, the thermometer reading being corrected for zero point. The numbers in the third column give the change in temperature due to the current. In order to render these more comparable, each one is divided by the time (in minutes) that the current was flowing. The results are given in the fourth column and express the change in temperature produced by the current flowing for one minute. Since the current was precisely the same for each determination, the constancy of these numbers shows the constancy of the heat capacity of the calorimeter and its contents, for temperatures ranging from $-1°.4$ C. right up to practically

0° C. In the determinations made with the oil in contact with the ice, this constancy was as marked, if not more so, showing that even in this case there was no melting of the ice below 0° C.

Neither is there any increase in the specific heat of ice as it approaches 0° C., as has been maintained by some investigators, but it remains a definite constant right up to the point at which the ice melts. That is, such is the case for this very pure ice, as is shown by the constancy of its heat capacity, and this is what would be expected since ice is a crystalline substance. As already pointed out, if there are impurities in the ice, even in very small amounts, an apparent increase in its specific heat may be produced by the lowering of the melting point of some portions of the ice.

TABLE II.

Heat Capacity of Ice.

Date.	Actual Temp.	Change in Temp.	Change in Temp. per Min.	
Feb. 19.	−0.48	.161	.0322	
	−0.24	.065	.0325	
	−0.36	.064	.0320	
Feb. 20.	−1.43	.160	.0320	With ice mantles in glass test-tubes.
	−1.03	.161	.0322	
	−0.73	.163	.0326	
	−0.23	.163	.0326	
	−0.09	.133	.0332	
	−0.02	.063	.0315	
Feb. 20.	−0.92	.160	.0320	Ice mantles filled with kerosene.
	−0.66	.160	.0320	
	−0.23	.160	.0320	

Effect of Mixing Water and Oil. — When the ice melts the resulting water is stirred up with the oil, and the question arises whether this mixture of the two liquids is accompanied by any thermal change. The point was subjected to experiment in the following way. About 300 c.c. of oil was placed in the calorimeter, and about 50 c.c. of water, the latter being in a large test-tube. A glass rod with a brass tip was placed within the test-tube so that the bottom could be broken from the outside. This arrangement was allowed to stand over night to assume an equilibrium temperature. The next morning a long series of temperature readings were

taken to make sure that the temperature was constant while the oil was thoroughly stirred. Then the test-tube was broken and the water allowed to flow out and mingle with the oil, and there appeared to be a rise in temperature of 0°.001 C. In order to see whether this was due to any action between the oil and water, or only to the mechanical energy put forth in breaking the tube, the motion of breaking the test-tube was repeated. In this case also there was a rise of temperature of the same amount as before, and since this time there was no question of water mixing with the oil, this increase in temperature, if real, must be due to the mechanical energy expended in the process of breaking the tube.

It is certainly safe to assert that there is no production or absorption of heat when the oil and water are stirred up together.

EXPERIMENTS WITH PURE ICE.

In view of the preceding investigations, all further experiments were made with ice formed from the pure distilled water. About half a liter of this water was boiled for twenty or thirty minutes to remove as much of the dissolved air as possible. The remaining 300 c.c. of water was poured into a beaker which it nearly filled, and when cooled somewhat the beaker was set in a freezing mixture of fine ice and salt. If supercooling of the water was allowed, ice crystals would suddenly form throughout the water. To prevent this form of ice and obtain it in a firm compact layer a small bit of ice was frozen to the side of the beaker just above the surface of the water, and below the surface of the freezing mixture outside. Here it would remain till the outer layer of water was cold enough to freeze. Then a film of ice was seen to start from the bit on the glass and spread over the inner surface of the beaker. After an hour or two this layer of ice reached a thickness of nearly a centimeter. The remaining water was discarded and the ice removed from the beaker by slightly warming the outside. This cup of ice was taken to the coldest place available and cut into cubes ranging from one to one fourth cubic centimeter in size. These pieces were dropped into another beaker in a freezing mixture where they were cooled several degrees below 0° C. and thus freed from any clinging water either on the surface or interior. The ice was then

weighed and quickly transferred to the calorimeter as already described.

Eight determinations of the heat of fusion of this ice were made on as many different days, one entire day being required to make one determination. During the greater part of this period the weather was below 0° C. in the morning which greatly facilitated the handling and weighing of the ice.

The amounts of ice used in each of these experiments are shown in the following table:

TABLE III.

Amounts of ice used in the experiments.

Date.	Weight of Ice.		
	Beaker + Ice.	Beaker.	Ice.
Feb. 27.	287.86	186.49	101.37
Feb. 28.	292.78	190.43	102.35
Mar. 2.	274.82	194.59	80.23
Mar. 3.	283.12	194.67	88.45
Mar. 4.	307.43	194.41	113.02
Mar. 5.	289.91	198.84	91.07
Mar. 6.	302.37	200.09	102.28
Mar. 7.	275.73	189.81	85.92

Determination of Heat Capacity. — With this very pure ice which it had been shown could be warmed right up to 0° C. without any melting or apparent increase of its specific heat, it was expected that the initial temperature could safely be brought to within a few tenths of a degree of 0° C. Nevertheless there appeared unmistakable evidences that if the temperature was too near 0° C. a small amount of melting of the ice would be caused by the current used in the preliminary experiments to determine the heat capacity of the calorimeter with the ice. Such determinations were, therefore, made only at temperatures below −1° C.

In the determination of the heat capacity, the current, as already stated, was that which would flow through the heating coil when its terminals were maintained at a difference of potential equal to the E.M.F., E, of one standard cell. The heat produced is then given by the formula E^2t/R, where R is the resistance of the coil and t the number of seconds that the current was flowing. The ratio of the heat generated by the current, expressed in joules, to the

change in temperature which it produced is termed the "heat capacity with ice."

The results of these determinations are given in Table IV. The mean temperature of the calorimeter and contents during each determination of heat capacity with ice is given in the second column. The third column shows the change in temperature produced by the amount of heat recorded in the fourth column. Often two or more determinations were made with the same ice at slightly different temperatures, the average of the results being used in the calculation of heat capacity, which is given in the fifth column in terms of the number of joules which would be required to raise the calorimeter and contents one degree.

The initial equilibrium temperature of the experiment for March 3 was too near 0° C. to allow a direct determination of heat capacity. Of course this could not be very different from the heat capacity on previous days as the same calorimeter and the same amount of oil was used each time, and approximately the same amount of ice. The determinations of February 27, February 28 and March 2 were all very satisfactory, and these are plotted as ordinates against the corresponding amounts of ice as abscissæ, and for the short range required of this curve it gives the heat capacity for varying amounts of ice very exactly. The heat capacity with ice for the experiments of March 3, 4 and 7 are obtained, then, from the determinations of the first three experiments by means of this curve.

The second part of Table IV. contains similar data for the heat capacity with water, obtained after the ice has been melted. A direct determination was made each day save the last, when the final equilibrium temperature was at the upper limit of the thermometer and higher temperatures could not be measured. The amount of ice used on this day was a mean between the amounts used on March 2 and 3, and as the same amount of oil was used, the heat capacity must be very closely the mean of the values obtained on these two days, and this mean is taken for the heat capacity.

Measurement of the Electrical Energy. — The details of the measurement of the E.M.F. and current used for melting the ice are given in Table V. As fully explained above, the current was measured by the use of a half-ohm coil and standard cell. The

TABLE IV.

Heat Capacity.

Date.	With Ice.				With Water.			
	Temp.	Change in Temp.	Joules.	Joules per Degree.	Temp.	Change in Temp.	Joules.	Joules per Degree.
Feb. 27	−1.40	.096			+.83	.140		
	−1.09	.097	62.75	650	+.86	.140	125.5	896
Feb. 28	−1.60	.194						
	−1.12	.193	125.5	648	+.95	.744	660.	887
Mar. 2	−2.00	.198						
	−1.54	.198			+.25	.152	125.5	
	−1.16	.198	125.5	634	+.40	.304	251.0	825
Mar. 3	from curve			640	+.22	.149		
					+.34	.150	125.5	839
Mar. 4	"	"		656	+.67	.705	660	935
Mar. 5	−0.60	.147	125.5	853	+.49	.609	618	1016
Mar. 6	−1.10	.142		890				
	−0.74	.141	125.5	638	+.87	.299	309	1034
Mar. 7	from curve				Mean of Mar. 2 and 3.			832

NOTE.—The amount of oil used on March 5 and 6 was 300 c.c. Hence the heat capacity is greater on these days than on the others when only 200 c.c. of oil was used.

values of the E.M.F. of the cell and the resistance of the coil, given in the third and fourth column, are those determined by the National Bureau of Standards. The value of the current thus determined is given in the fifth column.

The National Bureau of Standards also determined the value of the resistances Q and P, which were used as a potentiometer for the measurement of the E.M.F. between the terminals of the heating coil. Q was a box marked 10,000 ohms, while P was another box in which the resistance could be varied by one-ohm steps. The resistances used were nominally 1,113 ohms and 1,114 ohms, being that value which gave the nearest balance, the difference from a complete balance being measured by the deflection of the galvanometer. The mean deflection for each experiment is given in the eighth column, and the corresponding change in P necessary to give zero deflection is given in the ninth column. The voltage thus measured by the potentiometer is given the tenth column. The current through the heating coil is obtained by subtracting that in the potentiometer circuit from the total measured in the standard coil. The last column gives the amount of heat generated in the

TABLE V.

Measurement of the Electrical Energy.

Date.	Measurement of Current.				Measurement of Voltage.						Time Current was Flowing. t	Joules $W = E(I-i)t$.
	No. of Cell.	E.M.F. of Cell. e	Resistance of Coil. s	Current. $I = e \div s$.	Potentiometer Resistanccs.		Mean Galvanometer Deflection. d	Equivalent Resistance $r = d \div 8$.	Voltage $E = \frac{Q+P+r}{P+r}$	Potentiometer Current $i = \frac{E}{Q+P}$		
					Q	P						
Feb. 27	329	1.0193	0.50008	2.03736	9949.7	1,114.82	−1.00	−0.12	10.1175	0.00091	1,710	35,248
Feb. 28	"	"	"	"	"	1,113.79	0	0	10.1249	"	"	35,278
Mar. 2	"	"	"	"	"	1,114.82	−0.37	−0.04	10.1168	"	1,335	27,768
Mar. 3	"	"	"	"	"	"	−1.62	−0.20	10.1181	"	1,460	30,210
Mar. 4	"	"	"	"	"	"	−0.55	−0.07	10.1170	"	1,865	38,441
Mar. 5	328	1.0192	"	2.03716	"	1,113.79	+3.60	+0.45	10.1202	"	1,510	31,131
Mar. 6	329	1.0193	"	2.03736	"	1,114.82	−1.83	−0.23	10.1174	"	1,725	35,560
Mar. 7	328	1.0192	"	2.03716	"	"	−1.90	−0.24	10.1175	"	1,465	30,195

heating coil, being the continued product of the E.M.F., current and time.

That portion of this heat which was used in other ways than melting ice is calculated in Table VI. The second column gives the equilibrium temperature of the calorimeter and its contents just before the current was passed. This, multiplied by the heat capacity from Table IV., gives the heat required to raise the whole to 0° C., and is termed the "initial correction." The "final correction" is calculated in the same way, being the heat required to raise the calorimeter with water from 0° C. to the final temperature.

The heat lost (or gained) to the calorimeter by the combined action of radiation, conduction, convection, stirring, etc., is determined by the rate of cooling (or warming) experienced when no heat is being supplied by the current. These rates were observed and recorded in connection with the determinations of heat capacity. Expressed in joules per minute, they are plotted as ordinates against

TABLE VI.

Corrections.

Date.	Initial Temperature.	Heat Capacity with Ice.	Initial Correction.	Final Temperature.	Heat Capacity with Water.	Final Correction.	Average Temperature.	Radiation per Minute.	Duration of Experiment.	Correction for Radiation, etc.	Total Correction.
Feb. 27	−.788	650	512	+ .852	896	763	+.51	2.45	37	91	1366
Feb. 28	−.687	648	445	+ .642	887	569	+.41	2.10	33	69	1083
Mar. 2	−.987	634	626	+ .165	825	136	+.61	2.50	61	153	915
Mar. 3	−.709	640	454	+ .124	839	104	+.41	1.50	55	83	641
Mar. 4	−.535	656	351	+ .332	935	310	+.52	2.00	39	78	739
Mar. 5	−.468	853	399	+ .173	1016	176	+.61	2.20	37	81	656
Mar. 6	−.605	890	538	+ .693	1034	716	+.68	4.00	33	132	1386
Mar. 7	−.389	638	248	+1.298	832	1080	+.80	4.00	40	160	1488

the corresponding temperatures as abscissæ, and the resulting curve gives the loss at any temperature. The eighth column gives the average temperature of the calorimeter during each experiment, and the loss of heat by radiation, etc., at this temperature is read from the curve corresponding to that experiment and recorded in the ninth column. The loss of heat at a given temperature is not necessarily the same on different days, since the temperature of the

chamber D, Fig. 2, might have varied by several tenths of a degree.

The "duration" of the experiment is the interval in minutes between the reading of the initial and final temperatures and during which time the loss of heat is effective in reducing the value of the latter. The product of the loss of heat per minute by the duration gives the total amount of heat lost through these causes. The total correction given in the last column is the sum of these three corrections and represents that portion of the heat generated by the current which is not used in melting ice.

SUMMARY.

In the following table is given the final calculations for the heat of fusion. The numbers in the second and third columns are taken from Tables V. and VI., their difference being given in the fourth column. This is the amount of heat used in melting the quantity of ice in the fifth column, and the amount of heat per gram of ice is given in the sixth column. The last column shows the weight given to each experiment:

TABLE VII.

Summary of Results.

Date.	Total Joules.	Total Cor.	Cor. Joules.	Weight of Ice.	Heat of Fusion.	Weighting.
Feb. 27	35,248	1,366	33,882	101.37	334.24	6
Feb. 28	35,278	1,083	34,191	102.35	334.06	5
Mar. 2	27,768	915	26,848	80.23	334.69	3
Mar. 3	30,210	641	29,569	88.45	334.30	2
Mar. 4	38,441	739	37,702	113.02	333.59	4
Mar. 5	31,131	656	30,475	91.07	334.63	5
Mar. 6	35,560	1,386	34,174	102.28	334.12	5
Mar. 7	30,195	1,488	28,707	85.92	334.12	3
				Weighted mean =	334.21	
				Probable error of " " =	.08	

All of the determinations of the heat of fusion are seen to agree closely with the mean, the greatest variation being for the experiment of March 4. There is nothing to indicate why this value comes out thus far from the mean.

The probable error of the weighted mean, calculated by the method of least squares, is 0.08 joule, thus giving for the final value of the heat of fusion of ice

$$334.21 \pm 0.08 \text{ joules},$$

taking the value of the Clark cell as 1.434 volts at 15° C.

If there is an error of 1 part in 1,000 in the value of the Clark cell, it would alter this value of the heat of fusion of ice by 2 parts in 1,000, since the electrical energy has been calculated from the formula EIt, and both E and I are determined in terms of the standard cell, the E.M.F. of which is expressed in terms of the Clark cell.

This result can be expressed in terms of mean calories if we take the value for a mean calorie to be 4.1832 joules, as determined by Reynolds and Moorby,[1] and recently corroborated by Barnes.[2] This gives for the heat of fusion of ice

$$79.896 \text{ mean calories.}$$

Since the number 80 is familiarly used to express the heat of fusion of ice, it is interesting to note that $334.21 \div 80 = 4.1776$ joules, which is very closely the minimum specific heat of water as determined by Barnes. In other words, the heat of fusion of ice at 0° C. is

$$80.00 \text{ "calories at } 37° \text{ C."}$$

This value for the heat of fusion of ice is somewhat lower than that obtained by Hess, which appears the most trustworthy of previous determinations. However Hess stands alone in finding a value greater than 80 mean calories, or 336 joules, while the value here obtained is near the mean of all the previous determinations. It would be interesting to compare the heats of fusion of samples of ice prepared by different methods and of various degrees of purity. Some of the preliminary experiments with commercial ice seemed to indicate a slightly less heat of fusion, but these experiments will not warrant a more definite statement. However, for this very

[1] Reynolds and Moorby, Phil. Trans., Vol. 190, p. 381, 1897.
[2] Barnes, Phil. Trans., Vol. 199, p. 149, 1902.

pure ice, made as specified above, the value 334.21 joules in terms of the Clark at 1.434 volts at 15° C. will stand.

The writer's thanks are extended to Professor Ames for his interest and many helpful suggestions throughout this investigation. Valuable assistance has also been rendered by Mrs. Smith in that part of the work which required two observers.

JOHNS HOPKINS UNIVERSITY,
June 1, 1903.

THE CORONAL METHOD OF ESTIMATING ATMOSPHERIC NUCLEATION.

By Carl Barus.

1. *Introductory.* — With the advent of spring the values of atmospheric nucleation, if laid off on the same scale as in my earlier papers, have returned to the low order of values found last fall and the details of the temperature effect are obscured. I have supposed from this that temperatures below freezing are particularly favorable to high nucleation. One may note, moreover, that the distribution of atmospheric electrical potential is a maximum in winter and falls off in its yearly period in a way similar to the observed nucleation. The frequent occurrence of day minima in both cases should also be noticed.

The presence of maximum nucleation during the winter months when the maximum of dust contents is certainly to be anticipated for the summer, together with the fact that the numbers of nuclei found by the coronal method is much lower than the usual order of Aitken's[1] data, is worthy of remark. To produce coronas the nuclei must be very closely of the same size; for in a large trough, a rigorous uniformity of diameter of fog particle and possibly of distribution is implied, if the corona produced is to be sharp and brilliant. Particles of even slightly different sizes would give a blurred effect or a mere fog. Hence, as I understand it, the effect of ordinary dust vanishes from the corona and the nucleation observed is probably something much more definite. It is for this reason that in spite of very discouraging drawbacks, my interest in the subject has not waned, though I am well aware that the effect of chemical products of combustion in winter, such as sulphuric acid and the sulphides, has not been eliminated.

The subject as a whole has received enhanced interest in view of its relation to Arrhenius's interpretation of the geophysical impor-

[1] In the country after rain, 32,000; clear, 130,000; in cities, 100,000 to 500,000, etc. These data are about five times larger than the coronal values.

tance of cosmical, and in particular of solar dust. Some limitation has been put on the light-pressure theory by Schwarzschild, but this has rather stimulated Arrhenius to give a sharper expression to his views, and the theory now appears as the central figure of an elaborate treatise of cosmical physics.[1]

I have hoped therefore by aid of the present method eventually to add a contribution of my own. I have now nearly a complete record of the fall, winter and spring nucleations, though some of the early data will have to be repeated. There remain for completion the midsummer nucleations, which will be added in due time.

2. *Apparatus.*—This consists (Fig. 1) of a rectangular box, *AA*, 50 cm. long, 10 cm. in thickness and 10 cm. or preferably less in height, made of some material impervious to water. Wood covered while warm with a thick coating of wax and Burgundy pitch answers the purpose very well and is much lighter than (rigid) metallic vessels. The front and rear faces of the box are of thick plate glass. This must be kept clean on the inside and suitable scrapers with a vertical straight edge of soft rubber movable to and fro along the glass by aid of a long horizontal rod should be provided within the box. The rods pass out through perforated corks in the tubes *e*, with additional protection to secure an air-tight joint when not in use.

The air within the box communicates with the outside by three or more stopcocks, of which *B* is very wide (½ inch or more) in order that sudden exhaustion may be made through it. The stopcock, *C*, communicates with the atmosphere at a place free from local nucleation, through a length of ¼-inch lead pipe; *C* furthermore communicates with the interior of the box through a flat coil, *P*, of the same lead pipe, lying in the bottom of the trough, below the water level, *ww*. A coil of lead pipe in a water-bath may also be inserted on the outside of the box, the object being to heat the air to room temperature, especially in winter. Two thermometers, *T*, *T'*, with their bulbs respectively in the air and the water within the trough, register the temperature. The end of the influx pipe rises to a height, *q*, near the axis of the trough and opposite to the outlet *B*. Finally the whole inside of the trough is lined with a

[1] Lehrbuch der Kosmischen Physik, Vols. 1 and 2, Leipzig, Hirzel, 1903.

double layer of cotton cloth, *nn*, supported on a framework of stout copper wire. The trough should be mounted with its longitudinal axis on trunnions (not shown) in order that the whole interior may be moistened by a single rotation. The stopcock, *F*, provided with a cotton filter is often useful in testing.

The horizontal diameter of the coronas is observed, the point source of light being two meters off on one side and a suitable goniometer about one meter on the other side of the trough.

An ordinary jet pump suffices for aspiration (with the cocks *C* and *B* open), and with an added vacuum chamber provided with a

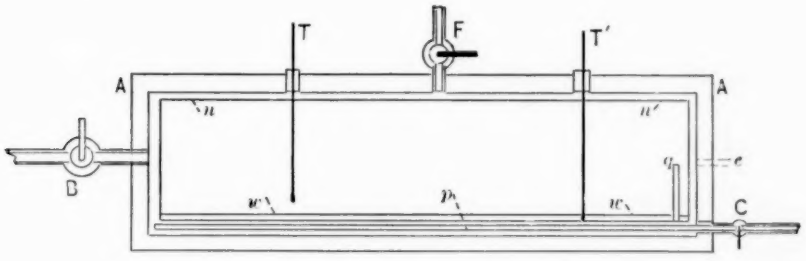


Fig. 1.

vacuum or mercury gauge, for sudden exhaustion (*C* and *B* having been closed), care being taken that the connecting tubing beyond *B* is wide.

3. *Diffusion from Two Surfaces.* — The high values of nucleation observed during the winter months will not be received without misgiving, since the air during the very cold weather is nearly dry and after being heated to 20° very far from saturation. Deficient saturation, however, would decrease the size of the fog particles and, *cæteris paribus*, increase the size of the coronas, in this way showing the same result as excessive nucleation. Hence it is necessary to estimate the time which is needed to saturate dry air in the apparatus described above.

Given a rectangular trough at the bottom of which is a surface of water, and at the top of which a surface of saturated cloth, the diffusion problem is equivalent to the case of an indefinite air plate into which the vapor enters on the two exposed sides. Thus if p be the vapor pressure relative to saturation at a distance x above

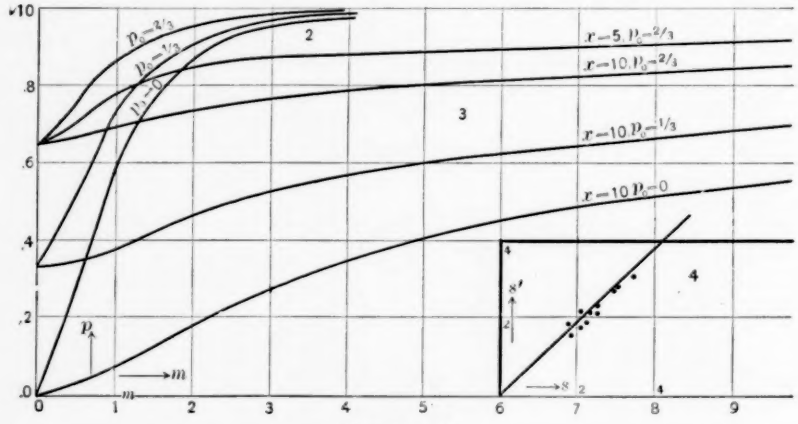
the surface of the water or $a - x$ below the wet cloth, at the time t,

$$p = 1 - \frac{4}{\pi}\left(\sin\frac{\pi x}{a}\varepsilon^{-(\pi/a)^2 kt} + \tfrac{1}{3}\sin\frac{3\pi x}{a}\varepsilon^{-(3\pi/a)^2 kt} + \cdots\right)$$

where k is the coefficient of diffusion.

If diffusion takes place into a partially saturated atmosphere at an initial pressure p_0 the factor $4/\pi$ in the last equation is to be replaced by $(4/\pi)(1 - p_0)$.

The question is most conveniently stated for the middle plane, $x = a/2$, inasmuch as the saturation here is least. If at 20°,



Figs. 2, 3, 4.

$k = .23$ cm²/sec, about, the graphs, Fig. 2, contain the relative saturation pressures, p, as ordinates, at the times given by the abscissas in minutes, according as the initial pressure is

$$p_0 = 0,\ p_0 = \tfrac{1}{3},\ \text{or}\ p_0 = \tfrac{2}{3}.$$

They imply successively greater saturation throughout. As the relative saturation left after any exhaustion would not probably be less than $\frac{2}{3}$ even in the absence of convection, the inferior limit of saturation shown by these data is excessive.

Thus, in 3 or 4 minutes the air-plate in the trough may be considered saturated under the most unfavorable conditions. In the aspiration of fresh air through the trough the maximum rate was about 4.5 lit/min, the usual rate 1.5 lit/min. The capacity of the trough, including parts above the cloth lining, about 11.3 liters.

Hence 3 to 5 minutes suffice to renew the air and each particle remains in the trough from 1.5 to 4 minutes. The time needed for adjustments after the influx pipe is shut off and prior to sudden exhaustion was about 1.25 minutes. Therefore the total time for saturation was 2.7 to 5 minutes, which should suffice even in case of diffusion alone.

The conditions, however, are much more favorable as the influx and efflux currents evoke considerable convection. The lightness of water vapor is itself favorable to the same end. It is observed, for instance, on exhausting *immediately* after the introduction of phosphorus nuclei, that filamentary condensation is in evidence, denoting currents upward axially and downward near the walls of the receiver.[1] These fog strands are the inevitable convection currents due to the lightness of the vapor.

4. *Miscellaneous Tests.*—If there had been undersaturation the coronas found on condensation should have been larger in less saturated, and smaller in more saturated parts, which was not observed. Sudden exhaustion immediately after shutting off the influx showed a somewhat enlarged uniform corona, but even enlargement was not invariable.

The nucleation obtained with the fast or the slow influx specified was without marked discrepancy for reasonable differences of time; *i. e.*, such as would not involve time losses of nuclei. Cf. § 6.

The effect of a long influx pipe (10 meters of ¼-inch lead pipe) and of a short pipe 1 meter long could not be sharply differentiated, owing in a measure to the cotemporaneous variations of atmospheric nucleation. So the presence or absence of the coil in the water-bath (24 turns each about 3.5 cm. in diameter) was of small significance.

Experiments were made with regard to the usefulness of this coil in keeping the influx air at room temperature, both by filling the bath with abnormally hot water (40° C.) and with broken ice. The effect on the apertures of the coronas was in both cases unimportant. Hence except on very cold days, the water-bath and coil may be withdrawn. The atmospheric air after traversing the 10 meters of influx pipe is already sufficiently heated to be introduced into the condensation chamber directly.

[1] Smithsonian Contributions No. 1373, 1903.

Tests were also made with a U-tube loosely filled with wet sponges and with a half-filled drying tube one meter long containing phosphorus pentoxide. In neither case was a definite effect on the coronas ascertained.

Freedom from leakage was finally tested by filtering the air. The coronas on sudden exhaustion showed a gradual decrease to complete evanescence.

5. *Diffusion from a Single Surface.* — The case is naturally less favorable if the upper wet surface (double cotton cloth) is omitted. The computation may be made from an expansion of Kramp's integral so that

$$p = 1 - \frac{2}{\pi}\left(\frac{x}{(4kt)^{\frac{1}{2}}} - \frac{x^3}{3 \times 1\,(4kt)^{\frac{3}{2}}} + \frac{x^5}{5 \times 2!\,(4kt)^{\frac{5}{2}}} - \cdots\right)$$

where p is the vapor pressure relative to saturation at a distance x above the surface of the liquid at the time t for the diffusion coefficient, $k = .23$. The curves are given in Fig. 3 for $x = 5$ cm. the middle plane, and $x = 10$ cm. the top plane of the trough. When the initial saturation is $1/3$ or $2/3$, the coefficient $2/\pi$ is to be modified as stated above.

If it were not for convection, therefore, such an apparatus would be unsuitable, for even after waiting five minutes, the air at the top, $x = 10$ cm., for an initial saturation of $2/3$ is but .8 saturated from diffusion alone. At $x = 5$ where the observations are made, the saturation is nearly .9.

One might therefore expect to obtain distorted coronas campanulate in outline, small below and large above, whenever condensation is produced within a few minutes after closing the inlet. Yet such is never the case if less than a minute is allowed after influx ceases. Granting that two to four minutes are needed on the average for a particle to pass through the trough, as stated above, if exhaustion is made immediately after closing the top layer is but two thirds saturated. Under these conditions there is in fact an unusually large green-centered, faint corona with a horizontal band of crimson color running through it. Half a minute later, however, the figure is quite regular again, showing that the convection of light vapor must be very active. After two minutes subsequent to the closing

of the influx pipe, the air may be regarded saturated except in the coldest weather.

6. *Absorption and Decay of Nuclei.*— The losses in the influx pipe are difficult to determine because of the variation of atmospheric nucleation. The observer is left in doubt whether a given difference is due to absorption in the pipe or to causes without.

The possibility of loss of nuclei on contact of dry air with the saturated gas in the condensation chamber is an independent question. It is also to be borne in mind that nuclei may possibly be produced by the sudden contact within the chamber. No evidence is forthcoming.

If the nuclei after being introduced into the receiver are solutions, some estimate of their persistence may be formed from my experiments on solutional nuclei, by treating the loss as if it occurred at the boundary of the vessel only. If the nucleation falls off from n_0 to n in the time t and k is the absorption coefficient,

$$n/n_0 = \varepsilon^{-2(1/r + 1/l)kt},$$

where r and l denote the radius and length of the cylinder in which absorption takes place. In case of comminuted pure water, $k = 5$ to 10 cm./min., and the nuclei should quite vanish in a few minutes. Thus if

$t =$ 1 min.,	$n/n_0 = .154$
2	.023
3	.003

If the nuclei are derived from very dilute solutions like river water an average value, $k = .1$ may be taken, whence if

$t =$ 1 min.,	$n/n_0 = .96$
3	.89
5	.83
10	.68
50	.15
100	.02

The reduction within three minutes will not exceed ten per cent., which would usually lie within a given type of corona. The datum $k = .1$ moreover corresponds closely to the values found for phos-

phorus and other nuclei. Hence if the type of corona changes after one or two minutes' waiting, it may be considered certain evidence that the air is not saturated and the diffusion error predominating. Owing to the difficulty of avoiding either insufficient saturation or excessive time losses my later observations contain data for two different aspirating currents, the faster corresponding to about three minutes' sojourn of the nuclei in the receiver, the other to a time longer than five minutes. In this way the effects of undersaturation which are most to be feared are guarded against, while the faster current gives data falling short of the absolute nucleation by not more than ten per cent.

7. *Effect of Pressure Difference.* — It is next to be considered whether the pressure difference, δp, used in the exhaustions is pronounced enough to catch all the nuclei. This is of particular interest in view of the presumably low order of nucleations obtained.

The usual value, $\delta p = 17$ cm. corresponds to the following pressure ratios and adiabatic temperature reductions in air $[(76 - p')/(76 - p' - \delta p)]^{.41} 273 = \vartheta'$ if p' is the vapor pressure of water and ϑ' the reduced absolute temperature.

10°.	Pressure ratio, 1.292	$\vartheta' = 254.7$
20	1.297	263.4
30	1.341	268.8

For comparison data were gathered with a larger pressure difference $\delta p = 22$ cm., for which the values are:

10°	1.414	245.5
20	1.416	254.1
30	1.432	261.5

Clearly the coronas for the larger temperatures and temperature differences must be smaller, *cæteris paribus*, in view of the greater quantity of moisture precipitated. The data for m, the quantity of moisture precipitated per cubic centimeter of saturated air have been computed by the method of C. T. R. Wilson and J. J. Thomson[1] and are given in the following table:

[1] Cf. Thomson, Phil. Mag. (5), XLVI., p. 538, 1898.

Table Showing Grams of Water Precipitated per cu. cm., m.

Initial Temperature.	Vapor Density.	Pressure Difference $\delta p = 17$ cm.			Pressure Difference $\delta p = 22$ cm.		
		Temperature Falls to		$m \times 10^6$	Temperature Falls to		$m \times 10^6$
		For Dry Air.	For Wet Air.		For Dry Air.	For Wet Air.	
°C.	$\times 10^6$	°C.	°C.		°C.	°C.	
10°	9.3	−18.3	− 4.5	3.7	−27.5	−10.1	4.2
20	17.2	− 9.6	+ 8.8	4.6	−18.9	+ 4.6	5.5
30	30.1	− 4.2	19.6	5.7	−11.5	+17.0	6.7

Since $m = n\pi d^3/6$, if there are n fog particles per cubic centimeter each of the diameter d, and since $sd = a$ where s is the aperture of the coronas with an arbitrary goniometer and a the corresponding constant,

$$s\sqrt[3]{m} = .806a\sqrt[3]{n},$$

which is constant for a given nucleation. Thus the relation between s' and s at $\delta p = 22$ cm. and 17 cm., respectively, may be written

$$s'/s = (m/m')^{\frac{1}{3}} = .952,$$

In Fig. 4 the line s'/s has been constructed and the observations grouped with reference to it. The cases are usually too low; or in other words at the higher pressure difference, $\delta p = 22$, which requires a longer period of waiting after influx ceases, relatively *fewer* nuclei are entrapped. From this one concludes not only that from the medium if saturated all the nuclei are precipitated at $\delta p = 17$ cm., but that at the higher pressure difference the time needed for adjustment is excessive and that the time loss of nuclei in the receiver frequently becomes appreciable.

8. *Precipitation per Cubic Centimeter.* — To determine m I originally proceeded as follows: In a mixture of x grams of vapor, y grams of air and $1 - (x + y)$ grams of water, the absorption of heat due to a rise of temperature $d\vartheta$ at constant volume was taken as $C[1 - (x + y)] + hx + r dx/d\vartheta + cy$, per degree, where C, c, and h are the specific heats of water, air at constant volume, and saturated vapor, respectively, and r the latent heat. Since $h - C = dr/d\vartheta - r/\vartheta$, h may be eliminated. Again the absorption of heat due to a volume increase dv, at constant temperature is, if γ is the heat ratio, $r(dx/dv)dv + y\vartheta c(\gamma - 1)dv/v$. If the expansion is adiabatic the total heat

absorption is nil and the equation thus obtained may be reduced eventually to

$$\frac{d}{d\vartheta}\{rx/\vartheta + [C(1-y)+cy]lg\vartheta\}d\vartheta + yc(\gamma-1)dlgv = 0.$$

As this is not a perfect differential I assumed that the relation of v and ϑ to be approximately that of air, $v^{\gamma-1}\vartheta = \text{const.}$, supposing that I could *subsequently* correct for the precipitated water by successive approximation. In this way one obtains at the beginning and the end of the exhaustion for any two temperatures ϑ and ϑ', using accents throughout for the latter case, after integrating, $x'/x = (\vartheta'/r')(r/\vartheta - lg\vartheta'/\vartheta)$, where $(x-x')/x$ is the mass ratio of precipitated liquid to the original vapor. In my work thus far the results were computed in this way and for $\delta p = 17$ cm. were at 10°, 20°, 30°, $m \times 10^6 = .59$, 1.13, 1.85 grams respectively, where the corrections for precipitated moisture have been applied and δp is an isothermal value. If δp, the observed pressure reduction, were treated adiabatically the corresponding values of $m \times 10^6$ would be .42, .76, 1.28.

The results for m so obtained will have to be rejected as they are much too small (probably because the pressure coefficients were overlooked) when compared with the results of experiments due to Schlagintweit and to Konrad. The latter found that a fog which limits visibility to about thirty paces contains about four grams of water per cubic meter, whence on the average $m = 4 \times 10^{-6}$, for instance. In the direct approximation of Wilson and Thomson (*l. c.*), the value of m is computed as an intersection, by making the m values compatible with the vapor density curve for water. The data calculated in this way have already been tabulated for the pressure difference $\delta p = 17$, in § 7, and they will be used in the following summary.

9. *Relation of Nucleation and Aperture.* — In the earlier work the nucleation n, was found directly from the apertures, s, of the coronas, standardized by aid of lycopodium spores. The experiment gave $ds = a = .002$, so that $n = s^3(6m/\pi a^3)$ followed for any value of s. The low values for m (§ 8) were inserted and a table prepared from which n could be read off for any temperature. At 20°, for instance $n = 180s^3$, etc.

Measurements of s would naturally be made as far as the inside of the red ring or the circumference of the (eventually) white disc: but in such a case they bring out very strong periodicity in the first place, and are soon subject to large errors due to the increasingly vague and washed outline of the disc. Hence measurement is more appropriately made to the dark blue ring which limits the green coronas or to the dark interior of the green ring in the crimson coronas. These lines are not only sharper but they reduce the periodicity. It is understood that with air nuclei and $\delta p > 17$ cm., the green corona is seldom exceeded. Otherwise it would be necessary to increase the uniform pressure difference against which there is no objection other than the increased practical inconvenience.

If the old data for m be replaced by the new (§§ 7, 8), and if measurement of the aperture s be made as far as the *outer* edge of

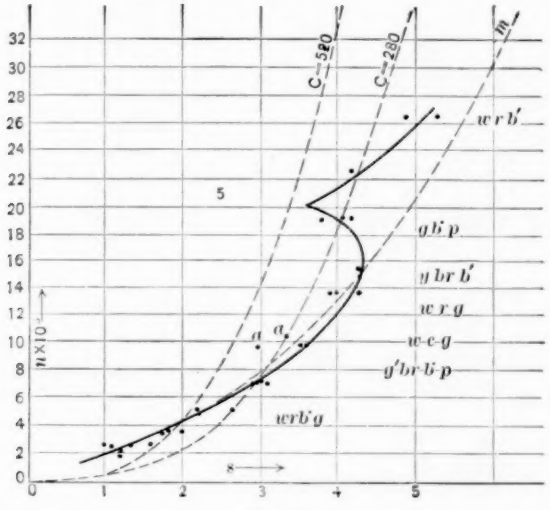


Fig. 5.

the red ring, and if the results of special optical measurements $sd = a = .0026$ be inserted, $n = 520 s^3$ at 20°, since $m = 4.6/10^6$. This equation, however, can have no application beyond the small normal coronas since for larger aperture s varies periodically with the number of particles or their diameters (Fig. 5).

Independent experiments were now made, and standardized by assuming arbitrary values of n for the two successive green

coronas obtained with air nuclei and computing the effect of successive exhaustion, geometrically.

The graph for these results, Fig. 5, shows the marked periodicity culminating in the green corona. Without doubt the corresponding crest should again appear in the lower greenish coronas [1] as in fact the two observations marked *a* seem to require; but the coronas are now so closely crowded together that the true contours of the graph are extremely difficult to obtain. Moreover the least imperfection of the filter in case of small *s* and *n*, adds particles whose number can no longer be regarded as negligible, while the occurrence of subsidence becomes increasingly more important and below $s = 1$ cm. wipes out the coronas at once.

Conclusion. — In the above paragraphs I have endeavored to present the complications to which the method of coronal registry of atmospheric nucleation is incident, complications which were not anticipated and for which I was altogether unprepared. In the course of my work I made an unfortunate error in endeavoring to reduce the data to absolute values; but, apart from this, the greater number of discrepancies (as for instance the periodic distribution of nucleation in terms of aperture) could not have been foreseen at the outset. Nevertheless, the practical convenience of the method makes it well worth while to attempt further development, and I hope therefore soon to carry out a complete revision[2] of coronas in relation to nucleation, embodying all the details I have since learned.

In addition to the remarks made in § 1, it seems to me that the variety and importance of the phenomena which are now attributed to the invasion of solar and cosmical dust into the atmosphere (such as certain variations of atmospheric pressure, of atmospheric electricity, of terrestrial magnetism, of auroral display, etc.), induce one to wonder why a continuous and systematic survey of atmospheric nucleation has not long since been included among the permanent records of observatories. Surely in discharging its remarkable and varied cosmical functions, this dust from afar must some day be detected undisguised and in the very act.

BROWN UNIVERSITY, PROVIDENCE, R. I.

[1] In Fig. 5, *w* denotes white, *b* blne, *c* crimson, *g* green *r* red, *br* brown, *y* yellow, *p* purple. A dot denotes dark (*b*· dark blue), a dash light or faint color (*g'* greenish). The coronas are entered at their mean height of nucleation.

[2] This has since been completed and will be published at an early date. (Am. Journ. of Sc., Oct., 1903, p. 325.)

DO FALLING BODIES MOVE SOUTH?

By Edwin H. Hall.

Part II.[1] Methods and Results of the Author's Work.

THE exact place of my experiments was the axis of the inclosed, isolated tower of the Jefferson Physical Laboratory of Harvard University. The distance of fall available in this tower is very small compared with the vertical distances which most of my predecessors in this research have used; but some compensation for this disadvantage is found in the fact that the tower is very well protected from disturbances due to winds or lateral inequalities of temperature, and in the further fact that in working within a laboratory I could have at hand all needed appliances and assistance.

The general plan and arrangement of apparatus is indicated by Fig. 1. At the top a wooden box *a*, having a trap door on one side and a window on the other, carries on its upper surface a contrivance of brass, in contact with which the ball to be released, a sphere of bell-metal 2.54 cm. in diameter, is held (see Fig. 2). The box *a* is supported by means of two strong joists, not shown, the ends of which rest upon and are fastened to a railing, which crowns the tower. Beneath *a*, and separated from it by a layer of cotton, is a square of board, *b*, having a central circular hole about 34 cm. in diameter, from the circumference of which is suspended a tube, *c*, of seamless cotton cloth ("pillowcasing") of the same diameter as the hole. The board *b*, is supported from the roof over the tower, and therefore has no rigid connection with *a* save by way of the base of the tower. The cloth tube, distended by five circular ribs of stout wire, extends downward about 20 m. to *d*, a rectangular opening, about 30 cm. by 40 cm., in the vaulted ceiling of the con-

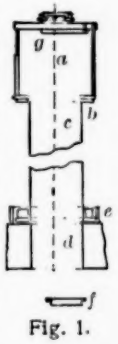


Fig. 1.

[1] A third paper on Preliminary Experiments, Variations of Method, etc., in the author's work will be published in the Proceedings of the American Academy.

stant temperature room, which is the lowest story of the tower. The lower end of *c* is attached to a square of board similar to that at the top. On this lower board are placed weights aggregating about 8 kgm. to keep the tube taut, and outside it, separated from it by a narrow space stuffed with fibrous material, is a casing of wood, *e*, to prevent lateral movement. Below *d* is shown, unsupported in the figure, a pan, *f*, filled with some mixture of tallow and beeswax, into which the ball is to drop. The board, *g*, now shown beneath the ball at the top of *a*, is a guard so placed as to arrest the ball if it is dropped prematurely, but free to swing around out of the way when the road is to be cleared.

Fig. 2, the scale of which is 1 to 4, shows in vertical section, and Fig. 3 shown in plan on the same scale, details of the releasing

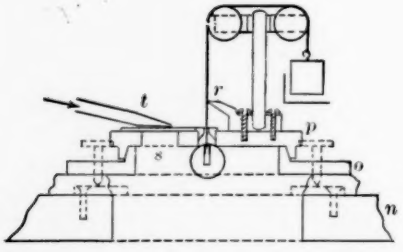


Fig. 2.

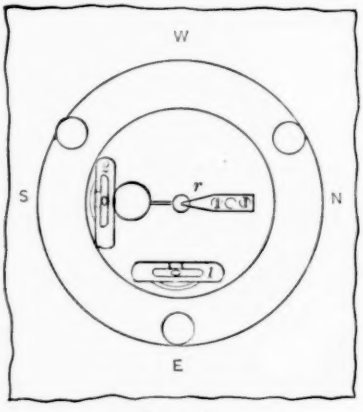


Fig. 3.

apparatus. In this figure *n* is the wooden top of the box *a* of Fig. 1. Screwed to *n* are three (two shown) brass disks, No. 1 having in its top a conical pit, No. 2 having a V-shaped groove extending toward No. 1, No. 3 being plane, which are the bases of the leveling screws supporting the brass plate *o*. The slight looseness of fit of these screws in the holes bored through the plate is very nearly abolished by the action of strips of spring brass attached to the under side of the plate. The arrangements thus indicated make this plate very firm in its place, while leaving it all desirable freedom of adjustment. On *o* rests the brass plate *p*, the two meeting in a conical bearing at which *o* furnishes the internal

and *p* the external part. To the upper surface of *p* is screwed a brass block having a beak *r*, against the tip of which rests the fine thread (or wire) which bears the weight of the ball before its release. Inclining slightly from the vertical at *r* this thread passes over pulleys to a counterpoise, which was usually a weight about 20 gm. heavier than the ball. The thread holds the ball close against the very slightly beveled lower end of a hole, 0.9 cm. in diameter, in the center of the plate *p*. The upper part of this hole is partly filled by a plug having a central hole 0.2 cm. in diameter through which the thread extends. The object of this plug is to prevent disturbance to the starting ball from the small gas flame, which, emitted in a puff from the tube *t*, is used to burn off the thread.

The lower end of the thread passes through a very fine hole in the top of a brass plug,[1] about 0.4 cm. wide and 0.5 cm. long, which is screwed into a hole bored about 1.4 cm. deep into the ball. This plug was drilled nearly through from the bottom, and the thin cap thus left at its top was punched through with a fine needle to admit the end of the thread, which after passing is knotted below so that it cannot be withdrawn. The object of making so deep a hole in the ball is to lower its center of gravity and so diminish the possibility of its rotation about a horizontal axis during fall. The method of placing the ball after the attachment of the thread was as follows: The gas tube *t*, and the thin brass plate lying just beneath it over the hole *s*, having been removed, the plug shown above the ball was lifted out of place and the free end of the thread passed through it and attached to the counterpoise; the ball was then lowered through the hole *s*, and brought to its place beneath the central hole, the thread passing along through a narrow slit, see Fig. 3, connecting the two holes; the plug was dropped into place, the thread adjusted in a certain file mark in the projecting beak *r*, and led carefully over the pulleys. As the thread was very slender, so as to be cut off instantaneously by the merest whiff of flame, the operation just described was attended by considerable

[1] Previous to May 28 a hard-rubber plug having a loop of thread projecting from the center of its top was used here, the upper thread being tied into this loop. Apparently the change of plug made no difference in the behavior of the balls, but the brass plug has a neater look.

risk of a premature and misguided fall of the ball, which, if not arrested by the guard shown in Fig. 1, might have done serious damage to the assistant or the apparatus below. Before release of the ball, the hole *s* and the slit leading from it were covered, as in Fig. 2, to lessen air currents. For the same purpose the space between the board *n* and the plate *o* was stuffed with cotton.

Fig. 3 shows, with some omissions, the transmitting apparatus as seen from above. The letters *N*, *E*, *S* and *W*, here mark approximately the points of the compass. The experimenter was always on the east side. In this figure 1 and 2 are the spirit-levels used in adjusting the plate from which the ball was to be dropped. As a rule, this plate was carefully leveled shortly before the dropping of each ball. On the side of each level is a curved projection, which I have called a loop, and each is here shown with its loop on the outer side, a condition designated as "loops out." Turning each level end for end we get a condition which is called "loops in." Substantially half the levelings were made with "loops out" and half with "loops in," the endeavor being in both cases to bring the ends of each bubble opposite two marks made on paper fastened alongside. The "beak" *r* is here shown pointing south, but by rotating the plate on which it rests the beak can be made to point north. Half the balls were dropped from "beak south" and half from "beak north," the change of position being made, usually, at the middle of each "pan" of six balls. The conical bearing of the upper plate *p*, Fig. 2, against the lower plate *o*, though very good, the two having been ground together, was not perfect, so that a slight rocking of the upper plate could occur, and after each turning of the upper plate through 180° a perceptible readjustment of the level was usually made. There was, apparently, no lateral relative movement of the two plates except through this rocking. With "beak north," level No. 2 was on the north side of the apparatus; but level No. 1 remained on the east side with both positions of the beak. The possibilities of error from faults of leveling have been carefully considered, but will not be discussed here.

Thus far, the only use mentioned for the beak *r* is to control the course of the thread which carries the ball; but in a different phase

of the work it fixed the position of the plumb-line, an even more important function, for the position of the ball just before release was determined almost wholly by the position of the central hole of the plate *p*, whereas the position of the plumb-line was determined entirely by the beak. As the object of the whole research was to find how far the center of the ball deviated from a vertical line during its descent, it was of prime importance that the mean position of the plumb-line, which mean was the vertical line used in the comparison, should pass as nearly as might be through the mean position of the center of the ball before release. The upper part of the plumb-line was usually a silk thread, though previous to May 23 it was sometimes a fine copper wire continuous with the main part of the suspension, and this thread or wire rested across the beak in the same file mark which at the proper time held the thread carrying the metal ball. Care was taken, with the help of devices not shown in the figures here given, to place this file mark very nearly over the center of the hole which fixed the position of the ball; but it was hardly possible to make this adjustment perfect, and even if it had been so the thickness of the suspending threads would have remained for consideration. Accordingly, the practice, already mentioned, of rotating the plate *p*, so as to make half the levelings and settings with "beak south" and half with "beak north," was adopted, with the reasonable hope of thus eliminating any appreciable error due to eccentricity of the beak. The change of position of the plumb-line caused by rotating the plate through 180° was, after releveling, much of the time less than one 0.02 cm. and sometimes less than 0.01 cm.

A few centimeters below the releasing plate the plumb-line became a bare copper wire 0.016 cm. in diameter, which previous to May 23 reached down to the bob, an ivory sphere (*B*, Fig. 4), nearly 6 cm. in diameter, immersed in water to check vibrations. On and after the date just mentioned a short piece of No. 38 copper wire, about 0.010 cm. in diameter, was used for the lowest part of the suspension, the coarser wire being rather too thick for satisfactory observation through the two very short range telescopes (see Fig. 7) used in reading its position. At first a smaller vessel than the jar *G* in Fig. 4 was used for holding the water sur-

rounding the plumb-bob; and in it the ball came to rest very promptly and remained very steady; but the danger of contact between the ball and the side of this small vessel led me to use finally the larger vessel here shown. Working alone in the room at the bottom of the tower, with the door closed, and avoiding hasty movements of my body, I had little trouble from vibrations of the plumb-line, and could make satisfactory observations of its position; but when inexperienced spectators were present in this room, I was obliged to defer this part of my work.

Figs. 4 and 5 show in some detail different aspects of the receiving apparatus. *W* is the wooden top of a low bench, which

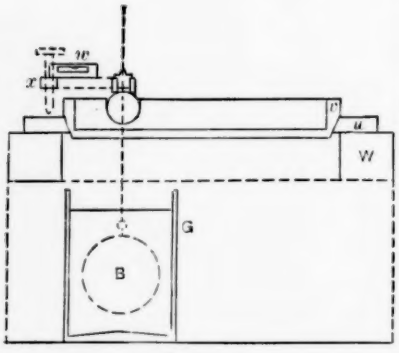


Fig. 4.

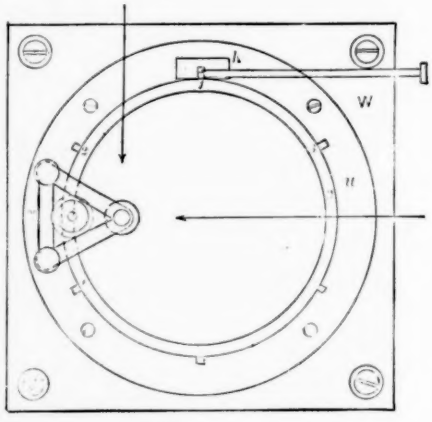


Fig. 5.

rests upon and is screwed fast to the top of a strong table, not here shown, which will be considered in connection with Fig. 7. Resting on *W*, and screwed fast to it, is a brass ring *u*, which supports, in a ground conical bearing, the brass pan *v*, which contains the mixture of tallow, etc., into which the ball falls.[1] Projecting from the side of the pan, Fig. 5, are brass studs, 1, 2, 3, 4, 5 and 6, by means of which, caught by the clamp *k*, the pan is secured in any one of six positions, in each of which it catches a ball. See Fig. 6, which is made from a photograph showing the balls dropped

[1] The tallow was, on and after May 15, scraped off level with the edge of the pan before use. Previous to the date just given the natural surface left by the cooling of the melted tallow, slightly descending from the edge toward the middle, was left undisturbed for the reception of the balls. This inclination may have increased very slightly the easterly deviation of the balls.

in one evening. To locate the vertical passing through the center of a ball after its lodgment in the tallow, a device indicated on the left in Figs. 4 and 5 was used, x being a tripod, two corners of which are supported by leveling screws resting on the brass ring u, while the third, through which is a vertical hole 0.9 cm. in diameter, is seated carefully on the firmly imbedded ball. Fitting accurately in the top of this hole is a brass plug ending above in a sharp point. On this point, after the tripod had been adjusted by the help of a small spirit level (w, Fig. 4), observations were made by means of the same telescopes which had been used to find the position of the plumb-line. Readings of the micrometer screws of

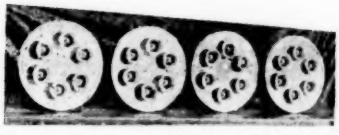


Fig. 6.

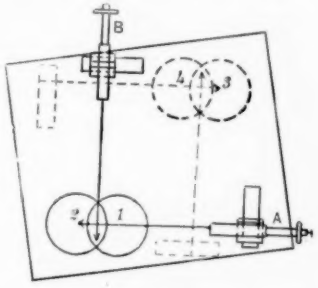


Fig. 7.

these telescopes were made and recorded to the nearest one hundredth of a turn, which corresponded nearly, in one case, to 0.002 cm., and, in the other case, to 0.001 cm., in the position of the vertical to be determined.

Each of the telescopes was carried by a stiff wooden support consisting of a V-shaped trough screwed to a block, which in turn was screwed to the table top (see Fig. 7). A is shown looking west[1] and B looking south, their lines of sight crossing at a point which indicates the position of the plumb-line. Circle 1 indicates

[1] The edges of the table in this figure correspond nearly to the points of the *compass*, while the lines of sight of the telescope are meant to be true east and west and north and south, respectively. The angle between the two sets of lines was at first laid out by compass, and not very accurately. It was about 10°. Toward the end of the investigation this matter received careful attention. A true north and south line was laid out at the top of the tower by Professor R. W. Willson, from an observation on the sun; and from this line by means of plumb-lines a line was placed at the bottom of the tower; but it was then too late to verify the angles used with all desirable precision, and a perceptible, though small, error in the final result may have been thus introduced.

the first position in which the receiving pan was placed, April 26 to May 28; circle 2, the second position of the pan, May 31 to June 17; circle 3, the third position, reached by turning[1] the table, and all which it carried, 180°, June 21 to July 8; circle 4, the fourth and last position, July 9 to July 25. The significance of these four positions requires, perhaps, some explanation. With the pan in position 1, for example, the ball falls much nearer to the western than to the eastern side, and this condition may affect somewhat the easterly deviation; for, on the one hand, the tallow is somewhat harder near the edge than near the middle, a circumstance which tends to push the ball toward the middle, while, on the other hand, the tallow sometimes crumbles and breaks away more on the side toward the near edge than on the other side, a circumstance which deflects the ball outward from the middle. Of these two opposing influences it is probable that the one acting toward the center is the more effective, on the average; but, as it was impossible to tell just how great the resulting error might be, it was necessary to make two series of experiments, one with the pan in position 1, the balls falling in its western side, the other with the pan in position 2, the balls falling in its eastern side. Later I turned the table through 180°, thus bringing the pan to its northeast corner, and made two more series of experiments, one with position 3, the other with position 4.

All of the balls of which account is here taken were dropped at night, work usually beginning between 7 and 8 o'clock and lasting about three and a half hours. Previous to May 26 the work at the top of the tower was usually done wholly by myself, the work at the bottom being done by an assistant, the routine being as follows: Leveling of top plate, with plumb-line hanging from beak (pointing south), readings of the position of the plumb-line by means of the telescopes at the bottom, reversal of the top plate to "beak north" position and releveling, readings on plumb-line after this reversal, removal of plumb-line, placing of ball at the top (Fig. 2) and pan at the bottom (Fig. 4), release of ball, measurement of position of ball in pan, etc. After all the balls for the night's use

[1] The crossing point of the arrows in Figs. 3 and 4 was made coincident with the crossing point of the arrows in Figs. 1 and 2 by moving the table toward the southwest.

had been dropped, the plumb-line was replaced on the beak, and observations similar to those at the beginning were made upon it.

On and after May 26 the routine was somewhat different: Leveling and reversing the top plate, with plumb-line on beak, by an assistant, readings of position of plumb-line by myself at the bottom, removal of plumb-line, leveling of plate and adjustment of ball by myself at the top, adjustment of pan at the bottom by the assistant, release of ball by myself, readjustment of pan by the assistant, etc. After three balls had been dropped into any pan, the release plate was reversed from "beak north" to "beak south" or *vice versa*, and the next three balls were dropped into it from the plate in its new position. After all the balls, usually twenty-four a night, had been dropped, the plumb-line was replaced on the beak, the assistant and I changed places once more, and observations on the plumb-line were made again. After this each pan in turn was put back into place, and observations were made by myself on the position of each ball, my assistant adjusting and leveling the tripod (Figs. 4 and 5).

Each pan of six balls was treated as a unit in calculations. That is, no fraction of a pan was counted. If any accident occurred which required the rejection of any one ball in a pan, the whole pan was rejected. The only accidents which were held to justify such rejection were those cases in which, before the use of the guard *g*, Fig. 1, a ball had been dropped unintentionally or had struck an obstacle in its fall, or had found the tallow too soft.[1] Mere divergence of a ball from the ordinary path, provided it had been dropped under apparently usual conditions, was not regarded as sufficient reason for excluding this ball, however great its divergence might be. There was no "criterion for the rejection of doubtful observations" based on the magnitude of supposed errors. The method followed was to bury exceptional cases beneath the weight of numbers. The extreme range north and south and east and west per pan was, on the average, about 0.5 cm., the improvement in this respect from April 26 to July 25 being very small, in spite of constant efforts to better the conditions of the experiment.

[1] With great heights tallow and beeswax would not do. I have tried lead, and find that it serves very well indeed. Each ball makes a clear imprint and rebounds only a little.

The results obtained, after much preliminary work ending April 23, are as follows:

Date, 1902.	Position of Pan.	Mean Deflection.		Probable Error.		No. of Balls.
		S	E	Of S	Of E	
April 26 to May 28	1 (Fig. 7)	0.0057 cm.	0.163 cm.	0.0083 cm.	0.0093 cm.	258
May 31 to June 17	2 "	−0.0038 "	0.149 "	0.0103 "	0.0093 "	210
June 21 to July 8	3 "	−0.0011 "	0.189 "	0.0070 "	0.0091 "	240
July 9 to July 25	4 "	0.0249 "	0.082 "	0.0101 "	0.0103 "	240
General means,		0.0050 cm.	0.149 cm.		Total =	948
Probable error, 1st		0.0043 "	0.0047 "			
" " 2d		0.0039 "	0.0146 "			

In taking the "general mean" each S or E was given a weight inversely proportional to the square of its probable error. The probable error marked "1st" of the general mean was reckoned from the four individual probable errors, on the assumption that the main differences between the deviations, for the four positions of the receiving apparatus, were not the result of casual errors, but were due to certain fixed conditions which the combination of the four positions would eliminate from the result. The probable error marked "2d" was reckoned on the assumption that each of the four positions should have given the same result. This latter assumption is, for reasons already shown, far from correct in the case of the easterly deviation, and accordingly it gives a large probable error for that case.

Cajori gives as the formula of Gauss for the easterly deviation, without air resistance,

$$E_1 D_1 = \tfrac{1}{3} \cos \Phi \, g u t^3.$$

where Φ is the latitude, g the acceleration of gravity, u the angular velocity of the earth on its axis, and t the time of fall. With this formula I get as the easterly deviation which should have been found in my experiments, if they had been carried out with perfect accuracy in a vacuum, 0.179 cm. The mean easterly deviation actually found in these experiments, 0.149 cm., differs 0.03 cm. from this theoretical value, a quantity too large to be accounted for by the resistance of the air. I attach but little significance to this discrepancy, as the conditions for determining the easterly deviation in my work were plainly not so good as those for determining the southerly deviation.

THE THERMO-ELECTRIC BEHAVIOR OF NICKEL NITRATE.

By Wm. McClellan.

History.

IN 1825 Walker[1] showed that when a hot and cold platinum wire are plunged in an electrolyte a current of definite directions results. In 1832 Faraday[2] repeated this experiment by putting two wires in the electrolyte and heating the portion around one and cooling the portion around the other. Gore,[3] in 1884, working with a number of electrolytes, showed that the potential difference depended upon the concentration of the solution. Bouty,[4] working with a number of electrolytes and different electrodes, found the thermo-electric power to be independent of the concentration. In 1887 Ebeling[5] did some very exhaustive work with copper and zinc electrodes in salts of these metals and found that the potential difference did depend upon the concentration. He also found that the maxima for salts of the same acid happen at the same temperature point and also that their maximum points correspond with the maxima of conductivity. Hagenbach,[6] in 1894, did the most recent and the best work in this line. He gives curves for a number of sulphates and chlorides with platinum and other electrodes which show considerable regularity. He states, however, his inability to get any satisfactory results with electrolytes containing nickel or nitrates. Bouty[7] tried nickel sulphate with nickel electrodes and found that an external current flowed from the cold electrode to the warm. Hagenbach repeated this experiment and found that no definite values were obtainable, the action being very erratic, some-

[1] Walker, Pogg. Ann., 4, p. 327, 1825.
[2] Faraday, Researches, Soc. 17, p. 1932, 1840.
[3] Gore, Proc. Roy. Sec., 37, p. 251, 1884.
[4] Bouty, Journal de Physique, 9, p. 229.
[5] Ebeling, Ann., 30, p. 530, 1887.
[6] Hagenbach, Ann., LIII., p. 447, 1894.
[7] Bouty. See before.

times positive and sometimes negative. With nitrates he could get nothing. Silver and cobalt salts are probably negative, but very inconstant. In view of these facts it was thought advisable to investigate the nitrates, particularly nickel nitrate, and see if any more definite results could be obtained.

ARRANGEMENT OF APPARATUS.

Fig. 1 shows this quite clearly. The solution was contained in the small beakers (*B*) and about 5 cm. deep and 3 cm. in diameter connected by a tube (*S*) of 15 mm. bore. The electrodes (*A*) were of platinum wire fastened to the ends of a light strip of wood (*T*), and continued out to (*J*). Here they were connected to copper wires which went to the potentiometer at *J*. The junctions were wrapped in thin silk and then wrapped close together in order that both junctions might be always at the same temperature. The

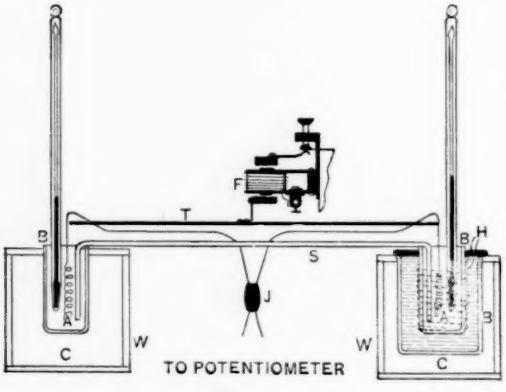


Fig. 1.

light stick (*T*) was fastened to one of the bars of an electrically driven tuning fork (*F*) which made about 256 V. S. The beaker (*B*) at the cool end was hung in a wood box (*W*) and packed around with raw cotton. The beaker at the warm end was hung in a larger beaker (*B1*) containing water and a heating coil (*H*). A current of 3 amperes in this coil would raise the temperature of the water from 20 to 80 degrees C. in one hour. The theromometers were ordinary mercury in glass thermometers calibrated for the work. Measurements of E.M.F. were made by means of a

Leeds Potentiometer. The latter was calibrated by means of a Weston Cadmium Standard Cell, with practically zero temperature correction. By moving a single switch on the instrument the contacts could be changed from the thermo-element to the standard cell. It was thus very convenient to test the calibration of the instrument after each observation of the thermo-element. The instrument was of the latest type and had means of reading to 0.000001 volt. The D'Arsonval Galvanometer used permitted readings to this accuracy, though usually it was purposely not done owing to the rapid changes in potential difference.

Solutions and Electrodes.

The solutions were all made with Kahlbach chemicals in pure distilled water free from ammonia. The nickel nitrate solution had 14.530 g. of crystalline salt to 500 c.c. of water. The solution was mixed cold and used in this way for the earlier curves. The solution was boiled for some later curves, and the concentration corrected by the addition of boiled water. As suggested by Hagenbach, the platinum electrodes were rinsed first in boiling water, then in nitric acid, then in water, then in alcohol, and finally heated to incandescence. This whole treatment is necessary after the electrodes have been used for even one curve. The writer tried omitting each part in turn, and found that it was not permissible. One must be particularly careful to make the nitric acid treatment complete. It is better to take one curve per day, leaving the electrodes in the nitric acid all the night before. If this is not convenient, boiling in nitric acid for about ten minutes is a good substitute. It seems more than likely that there is a slight deposit of nickel during the progress of the curve, and it is very necessary to dissolve this off entirely. The wire form of an electrode was used on account of its adaptability in design. In the earlier experiments it was thought desirable to have the electrode in the form of a small coil surrounding the thermometer bulb, so that the temperature of the electrode might be obtained as accurately as possible. When the vibrating method was attempted this was found impracticable, so the arrangement shown in Fig. 1 was adopted. The writer supposed that the size of the electrodes would have little to do with the resistance of the cell, *i. e.*, that the

resistance would depend chiefly upon the length and connection of the tube (*S*) (Fig. 1). Some preliminary experiments showed that this was not true. At one time, when the electrodes were straight wires, dipping about 5 mm. into the solution, a most uniform curve was obtained, but on testing the sensibility of the apparatus it was found to be enormously reduced. Were this not so, and it may not be so, for much larger electrodes, — say plates, then the same current would polarize the large plate much less and so eliminate the trouble to a greater extent. It may be well to say here that the writer has never worked with anything more sensitive than this cell. The greatest care had to be used in handling the potentiometer key. By holding the key down at the proper times the galvanometer could be deflected at will to show a positive or negative potential difference. Frequently when trying to adjust the potentiometer, the polarization seemed to keep just a little ahead of the adjusting, and it was necessary to make a big jump and so overreach it. This was particularly necessary when the electrodes had not been sufficiently treated with nitric acid. It was in trying to eliminate this trouble that the necessity for the complete acid treatment was observed.

Ebeling found,[1] as well as some others, that zero potential difference could not be obtained from two wires made in the same way and treated in the same fashion. Hagenbach[2] was unwilling to complete a curve unless this initial potential difference was less than 0.02 volt. The writer, in order to obviate this difficulty as much as possible, took a long piece of platinum wire and cut it at the middle. The resulting ends were then treated and used as electrodes. Towards the last it was found possible to set up the cell and have the initial potential difference as low as 0.005 volt regularly.

DETAILS OF EXPERIMENT.

The electrodes and solution were first prepared as has been described. Then the solution was put into the two beakers. One end of the tube (*S*) was suddenly thrust into one of the beakers, while the other end was depressed, which caused the solution to start immediately through the tube. The open end was then put

[1] Ebeling. See before.

[2] Hagenbach. See before.

into the other beaker. The electrodes and the thermometers were then placed in position and the whole allowed to stand until the temperature became steady. This sometimes took several hours, but usually if conditions were properly arranged, it was unnecessary to wait more than an hour. Contrary to expectation, it seemed rather difficult to get both solutions to exactly the same temperature, though the respective temperatures remained very steady.

The initial potential difference was now read. If found to be high, the electrodes were removed and again treated as described, and replaced. If the initial potential difference was below 0.02 volt, the temperatures were read and the heating current turned on. At first the temperature was caused to rise from 20° to 80° C. but as soon as the erratic nature of the curves was seen, the change was made from 20° to 50° C., as it seemed a waste of time to use the larger interval when the smaller gave no regular results. Some preliminary curves were taken by sending an exceedingly small current through the heating coil, and then waiting for the temperature to become constant; then increasing the current by steps and waiting each time. This method has several disadvantages, it takes many hours to get a curve, with the concentration changing during the interval, and changing more rapidly at the warm end than at the cold. This is particularly marked at the higher temperatures, when the evaporation is rapid. It was found to be much better to cause a larger current to flow through the heating coil, and measure the potential difference at different points as the temperature rose slowly. Usually the current was fixed so that there was a rise of about 30° C. per hour. The details of the vibrating electrodes, etc., will be given in the description of the curves.

The Curves.

(*a*) *Stationary Electrodes, Unboiled Solution.*

The curves shown are those obtained, Fig. 2, with stationary electrodes in unboiled solution; Fig. 3 with vibrating electrodes in unboiled solution; Fig. 4 with stationary electrodes in boiled solution.

The curves of Fig. 2 are very erratic. There was no difficulty in balancing the potentiometer, however, and the curves, such as

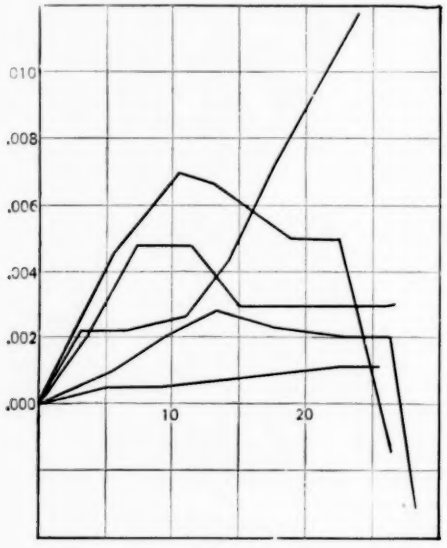


Fig. 2.

Data for Fig. 2.

$t = 23°$	$v = .002$	$t = 22.6°$	$v = .0098$	$t = 21°$	$v = .0085$
V	T	V	T	V	T
.0000	0	.0000	0	.0000	0
.0005	5.1	.0022	2.8	.0045	5.5
.0005	9.6	.0022	6.8	.0070	10.5
.0008	16.0	.0027	11.0	.0065	13.3
.0011	22.7	.0042	14.2	.0005	18.9
.0011	25.4	.0072	17.5	.0005	22.6
		.0101	21.5	−.0014	26.4
		.0122	22.9	−.0014	28.2

$t = 21.5°$	$v = .003$	$t = 22.5°$	$v = .005$
V	T	V	T
.000	0	.0000	0
.001	5.4	.0020	3.3
.002	9.3	.0042	7.1
.0027	13.3	.0047	11.3
.0023	17.7	.0047	15.2
.000	23.2	.0030	20.1
.000	26.3	.0030	23.5
−.003	28.3	.0030	26.1
		.0032	26.5

Data for Fig. 3.

$t = 25°$	$v = .003$	$t = 25.9°$	$v = -.008$	$t = 21.5°$	$v = .023$
V	T	V	T	V	T
.000	0	0	0	0	0
.003	1.6	.001	2.2	.001	1.5
.003	6.5	.001	3.1	.003	1.9
.006	9.6	.005	6.7	.003	3.9
.007	14.0	.005	8.7	.004	5.7
.011	17.9	.006	10.4	.002	10.8
.012	20.1	.029	11.4	−.002	13.8
.013	22.4			−.013	15.5
				−.015	18.3
				−.017	19.6

$t = 21.5°$	$v = .012$	$t = 19.5°$	$v = -.003$	$t = 23.8°$	$v = .003$
V	T	V	T	V	T
.0000	0	.000	0	.000	0
.0000	2.3	.005	4.15	.000	3.9
.0000	4.3	.006	9.25	−.001	9.0
.0000	6.6	.006	14.25	.000	13.0
.0005	7.7	−.008	20.65	−.0005	18.0
.0010	10.3	−.0051	23.35	−.002	22.3
.0010	14.2			−.011	28.1
.0010	17.6				
.0010	19.2				
.0010	21.4				

$t = 22.0°$	$v = .005$	$t = 22.6°$	$v = -.001$
V	T	V	T
.0000	0	.0000	0
.0005	5	.0000	2.0
.0015	12.3	.0007	10.0
.0015	14.0	.0014	11.5
.0010	17.6	.0014	15.6
.0000	24.9	−.0030	21.6
		−.0110	26.1

they are, were easily obtained. In all the curves, the positive sign means that the current flows in the external circuit from cold to hot. In plotting these curves I have plotted rise of temperature (T) as abscissas and change in potential difference (V) in volts as ordinates. At the heads of the columns, v indicates the initial potential difference in volts, and t the temperature at the cool electrode. The curves of Fig. 2 are all positive. A large number of curves taken earlier in the work show a far greater variation, partly negative and partly

positive. These are the best series that it was possible to obtain with stationary electrodes and unboiled solution. While taking a curve, the experiment of shaking the electrodes was tried just after balancing the potentiometer and the balance was greatly disturbed. This led to the experiment of vibrating electrodes.

(*b*) *Vibrating Electrodes, Unboiled Solution.*

Vibrating electrodes have been used very much in electrolytic deposition recently and have been found very advantageous, both vibrating anodes and vibrating cathodes. The writer has been unable to find any data relative to the particular application here employed, that is, very rapid vibration with very small amplitude. The amplitude used here varied from 1 mm. to 3 or 4 mm. — never

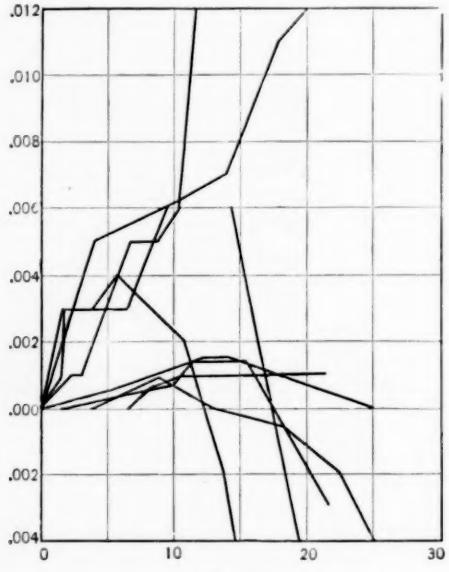


Fig. 3.

greater than this. Fig. 1 shows the arrangement sufficiently well. It is very evident that an electrode vibrating under these circumstances would of necessity stir in a more or less amount of air and this would certainly affect the result. The curves of Fig. 3 indicate two general groups, one in which the curves show a rapid rise in potential difference, and the other in which the rise is comparatively slow. Three of the curves show a change of several degrees

before any effect is noticeable and then the rise is sudden. In all the curves of the latter group it was noticed that there was little or no disturbance where the electrode pierced the surface, that is, there was little air stirred in, whereas in the former group, the electrode had considerable amplitude at the surface. The motion of the electrode was chiefly horizontal. Frequently on setting up the apparatus, the potential difference was read before starting the vibration, and again afterwards. The effect was quite sharply marked, If there was a great difference in the two readings, an examination

Data for Fig. 4.

$t = 24.3°$	$v = -.015$	$t = 22.8°$	$v = .0095$	$t = 21.9°$	$v = .0000$
V	T	V	T	V	T
0	0	0	0	.0000	0
0	1.9	0	4.8	.0000	2.4
.0004	6.1	−.0001	9.3	.0000	5.6
.0004	10.4	−.0001	11.2	.0001	9.4
.0006	15.3	−.0004	15.1	.0001	13.2
.0006	17.2	−.0005	18.8	.0001	16.6
.0009	21.1	−.0002	22.9	.00015	20.4
.0014	24.3	.0007	25.2	.00020	24.3
.0017	25.9	.0014	26.5	.00020	26.7
$t = 22.7°$	$v = -.001$	$t = 22.1°$	$v = .006$	$t = 22.9°$	$v = .0000$
V	T	V	T	V	T
.0000	0	.0000	0	.0000	0
.0000	6.9	.0002	4.5	.0007	6.1
.0000	9.9	.0002	8.5	.0020	11.3
.0009	14.2	.00035	13.9	.0012	15.5
.0037	18.9	.00050	20.5	.0005	18.2
		.00050	25.4	.0005	21.0
		.00050	28.6	.0005	22.8
				.0005	24.3
				.0005	25.3
$t = 23.3°$	$v = -.007$	$t = 24.1°$	$v = -.003$		
V	T	V	T		
.0000	0	.0000	0		
.0000	3.0	.0000	3.2		
−.0001	8.0	.0001	8.2		
.0001	12.0	.0001	12.2		
.0004	16.9	.0001	16.6		
.0004	20.9	.0005	18.5		
.0020	25.4	.0009	22.2		
		.0011	25.7		

of the electrodes showed considerable disturbance at the surface. When this was removed, the two readings were usually exactly alike, or else the reading with the vibration on settled down to a steady value though different from that with the vibration off. Many times when the balance with electrodes stationary was hard to obtain, the vibration was started and a reading taken easily, and on stopping the vibration the reading was found to be the same for some time, but would after a while become variable again. This was evidently due to the presence of free air in the solution, for no such trouble ever occurred with solution that had been boiled. When an unsteadiness in the potential difference was noticed in a boiled solution, it could be removed by again treating the electrode with nitric acid. Often no permanent balance could be obtained with the vibrator not working, but could be obtained when the vibrator was started. With too much motion at the surface, the difference in the two readings was sometimes 0.005 volt. No curves in any case were taken until the readings under same conditions were uniform for at least five minutes. Several times the cell was set up and readings of both kinds taken at intervals of an hour with little difference in the observations. It was absolutely necessary to keep the vibrating electrode from touching any part of the apparatus such as the thermometers or glass parts. When this occurred it was found impossible to balance the potentiometer.

(*c*) *Stationary Electrodes, Boiled Solution.*

These curves (Fig. 4) show a very marked improvement in regularity. Of the eight curves shown, two show a negative tendency, one of these, however, a very slight one, which soon becomes positive. All the curves show a very much smaller rise than had been found under other conditions. The solution was allowed to boil long enough to expel all air bubbles, and then put away to cool. Experiments were tried, by boiling the electrolyte in one of the beakers, and not pouring it at all, but simply allowing it to siphon over. This was tried several times, with the idea that probably the pouring from the vessel, in which the solution had been boiled, into the cell would introduce some air which in turn would cause some irregularity as in an unboiled solution. No marked improvement

could be observed, however, and the pouring evidently did little harm. It should be noticed that five of the eight curves show no change for at least three degrees' rise, and several of them for much more. Preliminary experiments with vibrating electrodes in boiled solution showed that the regularity would be much reduced by the vibration, probably on account of stirring air into the solution out

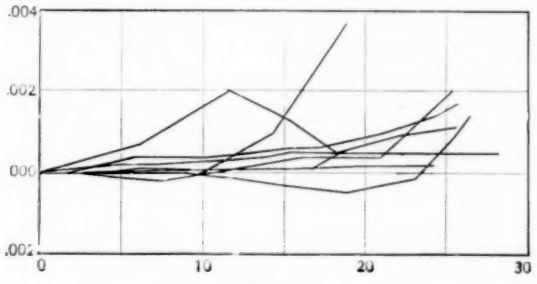


Fig. 4.

of which it had been boiled. No curves with this method were taken for this reason.

Summary.

From the final curves shown it is reasonably certain that the thermo-electric power of the electrolyte used is nearly zero. The extreme sensitiveness of the cell to polarization explains of course, the real trouble in the measurements. This accounts for the presence of positive and negative curves when the greatest care is taken in the observations. It is absolutely necessary to have the electrodes in a definite condition at the start. This is not to have simply a low initial potential difference, but because a high initial potential difference means the presence of nickel on the electrode, and this means ease of polarization, and consequently, non-uniform results. No amount of heating to incandescence would remove this. A prolonged bath in strong nitric acid is the only effective process. Unless the electrodes have been treated thus, the potential difference is not constant at constant temperature. That is, the changes in potential difference which take place due to the polarization on simply standing, are far greater than those due to the difference in temperature of the electrodes.

The effect of vibration of the electrodes is not entirely settled by this experiment. It is true, however, that in a solution which con-

tains air, the vibration has a steady effect, if the vibration does not stir more air into the solution. If any good result is to be obtained, the electrodes must be arranged with nodes at the surface of the electrolyte. A number of interesting effects might be obtained if a cell were designed in which the vibrations could take place without vibrating the surface, or if the part vibrating at the surface were insulated so that it did not form part of the electrode proper.

The presence of air in the solution causes great irregularity. Therefore boiling the solution free from air has a marked effect on the potential difference. Here the removal of the air shows the masking effect of the polarization much more easily. If the thermo-electric power amounts to anything it is probably positive, that is, it flows from cold to hot externally. In this connection it might be mentioned that the curves of sulphates and chlorides as shown by Hagenbach [1] show changes in potential difference of from 0.01 to 0.15 volt for a temperature rise of 60 degrees C., whereas an observation of Fig. 4 shows that in the case of nickel nitrate the rise would not be over 0.001 volt, even with polarization effects added on, or subtracted as the case may be. It is purposed to go on with this work until, if possible, an exact value is obtained for this effect whether zero or otherwise.

RANDAL MORGAN LABORATORY OF PHYSICS,
UNIVERSITY OF PENNSYLVANIA.

[1] Hagenbach, see before.

ON METHODS OF MEASURING RADIANT EFFICIENCIES.

By E. L. Nichols and W. W. Coblentz.

EVER since the publication of the paper of Julius Thomsen[1] on the Mechanical Equivalent of Light, in which he described the method of separating the luminous from the non-luminous energy of flames by the interposition of a water cell, it has been the general practice of students of artificial illumination to estimate radiant efficiency by some modification of his method.

It was the practice for many years to use instead of water a solution of alum, this substance being supposed to possess more complete opacity for the infra-red than water alone. Melloni is commonly cited as authority. Table II., however, on page 165 of the *Thermochrose;* in which the transmission of the rays of an Argand burner, by twenty-eight different liquids contained in glass cells is given, shows that according to his measurements such a cell containing a solution of alum transmitted 12 per cent. while the same cell when filled with distilled water allowed 11 per cent. only of the radiation to pass.

In 1893 Ernest Nichols,[2] who studied the transmission spectra of various substances in the infra-red, showed in a much more definite and conclusive manner the almost complete identity with water of the solution of both potassium alum and ammonium alum, as regards their transmitting power. Indeed in the case of solid alum it is doubtless to the water of crystallization rather than to the salt that the opacity is to be ascribed.

That neither water nor alum in solution, which may be regarded as identical with the former as regards its transmitting power, fulfills even approximately the conditions necessary to a screen for the separation of the non-luminous radiation has long been known, and it has been customary to make a correction for the imperfect opac-

[1] Thomsen, Julius; Poggendorff's Annalen, XXV., page 348.

[2] Nichols, E. F., Physical Review, Vol. I., p. 1.

ity of the water cell by means of a cell containing a solution of iodine in carbon bisulphide.

Evidence of the inadequacy of the correction obtained by this method and consequently of the inaccuracy of all our data, for the radiant efficiency of artificial light sources, that have been obtained by the method of the water cell has recently been afforded by Ångström.[1] He determined the radiant efficiency of the acetylene flame by an ingenious method in which the infra-red radiation is separated from the light-giving rays by the interposition of an opaque screen in the spectrum, after which the light is reassembled and its energy compared with the total energy from the flame. Ångström found for the radiant efficiency by this entirely independent method the value .056 which is a little more than half as great as that obtained by Stewart and Hoxie with a similar flame by the use of the water and iodine cells.[2] He finds that all radiant efficiencies hitherto published, with the possible exception of those given by Langley,[3] who made use of the method of plotting the energy curve for a source of light and of integrating the areas of the luminous and non-luminous portions, are much too large.

A recent investigation by one of the present writers[4] of the infra-red spectrum of iodine both in the solid form and in solution shows conclusively that the assumption upon which the use of iodine solutions for the purpose of determining the correction which it is necessary to apply to the infra-red radiation transmitted by the water-cell, namely, that iodine solutions which are of sufficient concentration to absorb completely the rays of the visible spectrum are fully transparent in the infra-red, is unwarranted. It is our purpose in the present paper to determine as definitely as possible the degree of inaccuracy involved in the usual method of measuring radiant efficiencies; and with this end in view we have made the following measurements.

RADIANT EFFICIENCY BY THE METHOD OF THE WATER CELL.

A cylindrical acetylene flame from a single-tipped burner was mounted and the deflection of a Nichols radiometer with rock-salt

[1] Ångström, Astrophysical Journal, Vol. XV., p. 223, 1902.
[2] Nichols, PHYSICAL REVIEW, Vol. XI., p. 215, 1900.
[3] Langley, Philosophical Magazine, 30, p. 26, 1890.
[4] Coblentz, W. W., PHYSICAL REVIEW, 16, p. 35, 1903.

window was noted when exposed to the direct radiation of this flame; also when a water cell with thin glass walls and containing a layer of distilled water 1 cm. in thickness was interposed; and finally when an iodine cell was placed behind the water cell so that the rays had to pass through the two. The iodine cell was of the same dimensions as the water cell. It contained a solution of iodine in carbon bisulphide of such concentration that the filament of an incandescent lamp viewed through the cell was just on the point of extinction. The result of five series of readings was as follows:

TABLE I.

Deflections (Direct).	Deflections (Through Water Cell).	Deflections (Through Both Cells).
15.40	2.29	0.98
15.25	2.30	1.00
15.35	2.29	0.99
15.22	2.29	1.03
15.28	2.36	1.00
15.300 (mean)	2.306 (mean)	1.000 (mean)

The energy transmitted by the water cell according to the above data is $2.306/15.300 = .1507$.

The proportion of radiation passing both cells as compared with that of passing the water cell alone is $1.00/2.306 = .4337$.

The radiant efficiency of the flame computed in the usual manner would be $.1507 \times (1 - 0.4337) = .086$. This value is somewhat lower than that obtained by Stewart & Hoxie (.105) for a flame of the Naphey type. That the flame of burners of the Naphey type is in general whiter than the flame from the single-tipped burner and presumably therefore is of a higher temperature and of a somewhat higher radiant efficiency, has already been shown by means of a spectrophotometric examination of these flames.[1]

RADIANT EFFICIENCY BY INTEGRATION OF THE ENERGY CURVE.

The energy curve of the spectrum of the flame was obtained by using a mirror spectrometer with rock-salt prism, and measuring the intensity of radiation throughout the spectrum by means of the

[1] Nichols, Jour. of Franklin Inst., Vol. 150, p. 350, 1900.

Nichols radiometer. This curve of observed intensities was corrected for slit-width variations by means of Paschen's equation:

$$af(x) = F(x) - \tfrac{1}{6} F_1(x).$$

A further correction was likewise introduced on account of the selective character of the transmission by the rock-salt window which was found to be much less transparent for the shorter than for the longer wave-lengths.

In making these measurements of the spectrum a set of three readings was obtained for each of a series of wave-lengths, one reading for the total intensity of the spectrum at the point in question, one for the radiation transmitted through the water cell and one for the radiation transmitted through the iodine cell; these being placed separately and successively between the flame and the spectrometer slit. The curve of intensities when no cell was interposed is plotted both in the observed and corrected form in Fig. 1.

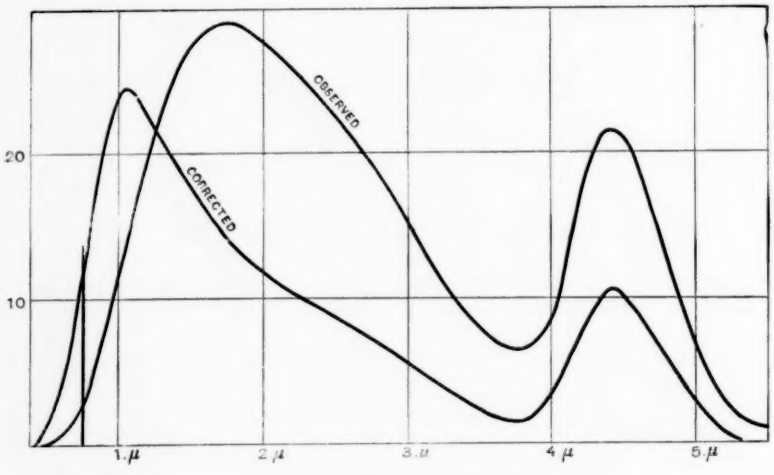


Fig. 1.

In this diagram the area included within the limits of the visible spectrum is separated from the infra-red by the vertical line at 7.6 μ. It is evident from a glance at these areas that the value for the radiant efficiency calculated by the method of a water cell is very much too large. The integration of the corrected curve, by means of a planimeter, gave as the ratio of the area of luminous energy to that of total energy, 0.033.

Transmission Through Water and Iodine.

The transmission of the water cell and of the iodine cell computed from the readings described above is shown in Fig. 2, in

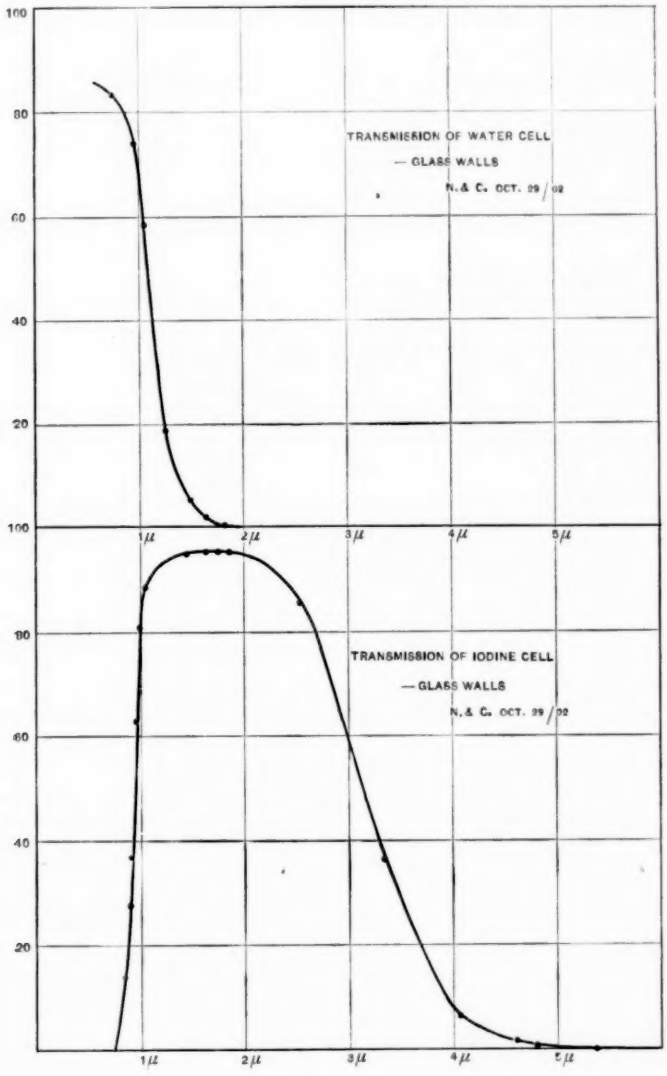


Fig. 2.

which ordinates give the percentage of transmitting power. Complete transparency would be represented by 100, complete opacity by zero.

It will be seen that the water cell, so far from being opaque throughout the infra-red, transmits 67 per cent. at wave-length 1 μ and becomes completely opaque only when wave-length 1.8 is reached. The iodine cell, the function of which in the determination of radiant efficiency, is based upon the assumption that it will transmit whatever infra-red radiation may pass through the water cell, is likewise very far from fulfilling this condition : for although

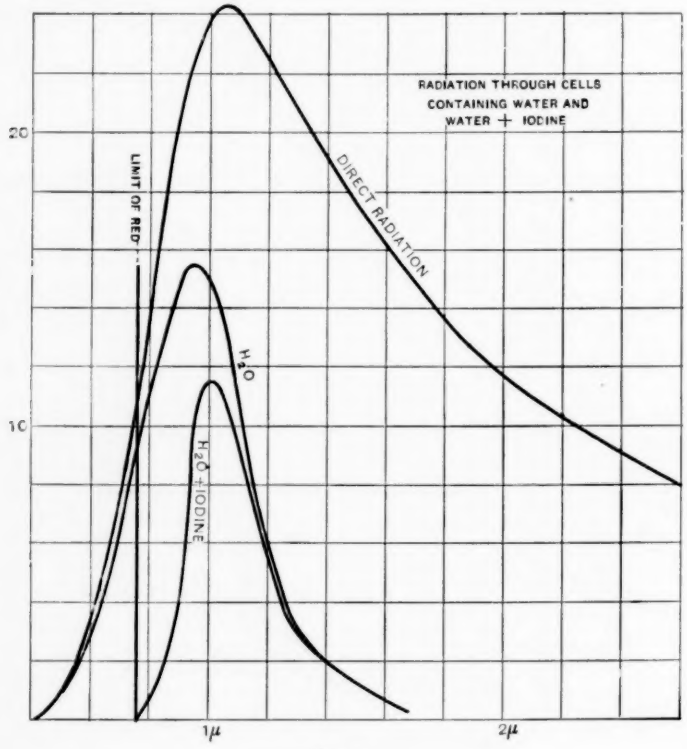


Fig. 3.

the transition from opacity to transparency is very rapid, the solution does not become fully transparent until the region 1.5 μ is reached.

The precise significance of the data obtained by the water cell method and the complete failure of the iodine cell to serve as a means of correcting for the transparency of water in the infra-red is better shown in Fig. 3, in which a portion of the energy curve for the flame is reproduced. The energy transmitted by the water cell is

shown by the curve marked H_2O and that portion of the energy thus transmitted which gets through the iodine cell when the latter is placed behind the water cell is shown by the curve marked H_2O + *iodine*. These curves indicate clearly the very large error involved in the use of the iodine cell and lead us to the rejection of it altogether in the study of radiant efficiencies.

An integration of the area of luminous energy and of the total energy enclosed in the curve marked H_2O shows that of the energy transmitted by the water cell only about one sixth, more accurately 0.174, falls within the visible spectrum. A comparison of the area measuring the luminous energy transmitted by the water cell with the area representing the total luminous energy of the flame shows that the losses of luminous energy by reflection and transmission in passing through this cell amount to 13 per cent. In order to check this ratio, the precise determination of which with the radiometer is somewhat precarious on account of the small deflections obtained in the visible spectrum, two similar acetylene flames were mounted at the end of a photometer bar and the ratio of their intensities was determined. The water cell was then placed in front of one of them and the measurement repeated. The transparency of the water cell thus measured was found to be 0.87, a value which is in agreement with that obtained by the integration of the curves. The computation of the radiant efficiency of the flame from the data obtained by the integration of these curves gives us $.1507 \times .174/.87 = .030$; a value somewhat smaller than that obtained from the integration of the energy curve itself.

Both of these values for the radiant efficiency are surprisingly low. The method of integrating the energy curve for the determination of the ratio of luminous energy to the total area, while it appears at first sight to be the most direct and therefore likely to give the best value, is unfortunately subject to considerable errors arising from the difficulty of making the proper correction for variations of slit-width within the luminous area. The radiometer readings for this portion of the spectrum are always small and the change in the slit-width, measured in wave-lengths, undergoes its most rapid changes as we pass over from the visible to the invisible spectrum. Uncertainties concerning the trend of the dispersion

curve of the prism which are apparently insignificant affect the ordinates of the corrected energy curve in this region very notably. Fortunately we have the means of checking the accuracy of the results in this case by comparing the ratio of the area of the curve representing the total transmission of the water cell to that of the total area of the energy curve with the corresponding ratio obtained from the measurement of the deflections due to the unrestricted and undispersed radiation from the flame as compared with the deflections produced when the water cell was interposed in the path of the rays. The ratio of the areas by integration is 1.528 while that obtained by the latter method is, as we have already seen. 1.507. The close agreement of these ratios is such as to give confidence in the substantial accuracy of the two methods.

An explanation of the low value of the efficiency of this flame is to be found in the peculiar character of its energy curve. It will be noted that the emission band at 4.4 μ is very large. The area of the region lying between 4.0 μ and 4.9 μ is in point of fact .192 of the total area of the curve. Integration of the corresponding area of a curve for a cylindrical flame measured by G. W. Stewart and corrected by Paschen's equation shows for the corresponding region an area of .0948 as compared with the total area of that curve. The radiant efficiency of Stewart's flame as determined by integration of the luminous and total areas gives .040.

The existence of a very powerful emission band at wave-length 4.4μ is characteristic of flames of the Bunsen type; as may be seen from the curve given by Stewart [1] in his paper on the spectrum of the acetylene flame. The very large emission band at this wave-length, in the case of the flame upon which the measurements described in the present paper were made, probably indicates that the air ducts in the burner were too large as compared with the orifice through which the acetylene gas reached the tip.

A reduction in the supply of acetylene from partial clogging of the orifice would tend to convert the flame into the Bunsen type and to reduce its radiant efficiency. Measurements of the spectrum of the flame on this particular burner made on different days, show that the intensity of the band varies through a wide range.

[1] Stewart, PHYSICAL REVIEW, Vol 13, p. 272, 1901.

The ratio of the deflection at the top of this band to that at the maximum of the energy curve on April 26 was .40. On October 29, the day of the readings from which the curve shown in Fig. 1 was plotted, it was .76. On November 8 it was .50; on December 13 it was .59.

Measurements with the water cell at a time when this ratio had a value of .50 gave for the ratio of the transmitted to the direct radiation .200 as against the .150 of Table I. The radiant efficiency on that day, assuming the ratio for luminous to total transmission through the water cell not to be appreciably changed, was $.174 \times .20/.87 = .040$. This value is identical with that obtained by integration of the curve for Stewart's flame.

We have in these observations without doubt an explanation of the principal difficulty of using the acetylene flame as a primary standard in photometry. The gas orifice in all existing types of the acetylene burner is very small and the partial clogging of this orifice would serve to modify to a serious extent the quality and the total brightness of the flame.

Conclusions.

The observations recorded in this paper suffice to show the very large correction which must be applied to the ratio between the energy transmitted by a water cell and the total radiation if one is to obtain a correct value for the radiant efficiency of any source of light; also the futility of the method of ascertaining this correction by means of the iodine cell. It is obvious that the factor by which this ratio must be multiplied will vary with the radiant efficiency, and while the nature of this variation is perhaps comparatively simple in the case of sources in which the light is derived from incandescent carbon the value of the correction for other sources can only be ascertained when we have definite information concerning the character of the infra-red spectrum. Were we to apply, to take an extreme case, the method of the water cell to the radiation from sodium vapor at the temperature of the electric arc we should have to do with an infra red spectrum nearly all of the energy of which lies, according to the curves published by Snow,[1] at wave-lengths less than 1.4 μ. The infra-red spectrum of this vapor consists chiefly of

[1] Snow, Physical Review, Vol. I., p 95.

two strong lines or pairs of lines at .82 μ and 1.13 μ. There is, it is true, a group of lines or bands lying between 1.7 μ and 1.9 μ but the energy of these is insignificant. What may lie beyond 2 μ we do not know. A very large part of the energy of such a source of light would presumably be transmitted by the water cell. In the case of the Arons mercury lamp according to measurements reported elsewhere by Coblentz and Geer[1] we have in the infra-red a group of lines which lie within the region of transparency of the water cell and a second group to which the cell would be entirely opaque. In the study of such sources of light determinations of the energy transmitted by the water cell are without significance. Results obtained by exploring the entire spectrum and plotting the energy curve would, on account of the difficulty of making a satisfactory correction for the variations of slit width, likewise be of doubtful value. To determine the radiant efficiency of such sources we must have recourse to a method based upon the principles used by Ångström in his recent paper or we must find some screen which will be opaque to the entire infra-red, and the transmitting power of which for the visible spectrum can be determined photometrically.

PHYSICAL LABORATORY OF CORNELL UNIVERSITY,
December 24, 1902.

[1] Coblentz and Geer, PHYSICAL REVIEW, Vol. XVI., p. 279.

NOTE ON THE DEDUCTION OF STEFAN'S LAW.

By Edgar Buckingham.

BOLTZMANN'S thermodynamic deduction of Stefan's law, that the total temperature radiation of a black body is proportional to the fourth power of the absolute temperature, is very concise; and it seems to me that the same thing may be done a little more clearly, at least for those who are familiar with the free energy principle.

Starting with the two laws of thermodynamics,

$$\delta\varepsilon = \delta Q + \delta W, \tag{1}$$

$$\delta\eta = \frac{\delta Q}{\theta}, \tag{2}$$

we have, for reversible processes,

$$\delta\varepsilon = \theta\delta\eta + \Sigma X\delta x, \tag{3}$$

where ε = internal energy, η = entropy, θ = absolute temperature, δQ = heat added to the system, δW = work done on the system, and $X_\kappa\delta x_\kappa$ = the part of the work due to the increase in the variable x_κ. If the system has only two degrees of freedom, and the only external force is a pressure, equation (3) takes the familiar form

$$\delta\varepsilon = \theta\delta\eta - p\delta v. \tag{4}$$

Let

$$\psi = \varepsilon - \theta\eta. \tag{5}$$

Then

$$\delta\psi = -\eta\delta\theta - p\delta v. \tag{6}$$

In any isothermal process, $\delta\theta = 0$, hence

$$\delta\psi = -p\delta v, \tag{7}$$

i. e., the work done on the system by the pressure p, during an increase of volume ∂v, is equal to the increase of ψ.

The function ψ is Helmholtz's "free energy," and equation (7) may be read: "The work done by a system during an isothermal, reversible change of state, is equal to the decrease of its free energy." This "free energy principle" is generally expressed simply by the equation which defines ψ, namely,

$$\psi = \varepsilon - \theta\eta, \tag{8}$$

which is then to be read: "The decrease of the free energy of any system during a reversible, isothermal change of state, is equal to the decrease of the total energy, minus its absolute temperature multiplied by the decrease in its entropy."

From equation (6) it is evident that

$$-\eta = \frac{\partial\psi}{\partial\theta}, \tag{9}$$

so that (8) may be written in the familiar form

$$\psi = \varepsilon + \theta\frac{\partial\psi}{\partial\theta}, \tag{10}$$

the interpretation in words being similar to that given above. So much for our thermodynamic tools.

The assumption to be made in the deduction of Stefan's law is one drawn from the electromagnetic theory of light; namely, that parallel rays falling on a black surface exert on it a pressure equal to φ, the volume density of energy in the incident beam. For an entirely unordered radiation inside a black enclosure of absolute temperature θ, the pressure at any point of the bounding surface will then be $\frac{1}{3}\varphi$; just as in the kinetic theory of gases we imagine our molecules, for simplicity, divided into three groups moving at right angles, each group having the sum total of the components in its direction of the momenta of all the molecules with their actual distribution of momentum, and then compute the pressure on that basis.

Consider a cylindrical enclosure formed of black surfaces, having a volume v, and provided with a frictionless black piston. Let the

uniform temperature of the walls be θ, and let the density of the unordered radiation inside be φ. Let the piston move out, reversibly and isothermally, so that the volume increases by δv. The work done by the system is

$$p\delta v = \tfrac{1}{3}\varphi\delta v, \tag{11}$$

which is, accordingly, the decrease in the free energy of the system, or the ψ of equation (10). We therefore have

$$\frac{\partial\psi}{\partial\theta} = \tfrac{1}{3}\frac{\partial\varphi}{\partial\theta}\delta v, \tag{12}$$

δv being arbitrary and independent of θ.

The temperature having been kept constant, the density of energy has not changed. Therefore, a quantity of energy $\varphi\delta v$ is now inside the enclosure, in addition to what was there at first. The internal energy of the system has, therefore, *decreased* by $(-\varphi\delta v)$, which is, therefore, the value of the second term in equation (10). Substituting these values in equation (10), we at once get the equation

$$\tfrac{1}{3}\varphi\delta v = -\varphi\delta v + \tfrac{1}{3}\theta\frac{\partial\varphi}{\partial\theta}\delta v,$$

or

$$4\varphi = \theta\frac{\partial\varphi}{\partial\theta}. \tag{13}$$

From this we have, at once,

$$\frac{d\varphi}{\varphi} = 4\frac{d\theta}{\theta},$$

and

$$\varphi = C\theta^4. \tag{14}$$

Those who do not like the free energy principle, may get the result by using the familiar thermodynamic relation

$$\left(\frac{\partial\eta}{\partial v}\right)_\theta = \left(\frac{\partial p}{\partial\theta}\right)_v, \tag{15}$$

which is evident from equation (6). The heat added to the system during the change of volume is equal to the increase of the internal energy $\varphi\delta v$, plus the work which the system has *given out*, namely

$p\delta v$, or $\frac{1}{3}\varphi\delta v$. Therefore, during the isothermal change, the total amount of heat added has been

$$\delta Q = \tfrac{4}{3}\varphi\delta v\,;$$

whence

$$\delta\eta = \frac{\delta Q}{\theta} = \tfrac{4}{3}\frac{\varphi}{\theta}\,\delta v,$$

and

$$\left(\frac{\partial\eta}{\partial v}\right)_\theta = \tfrac{4}{3}\frac{\varphi}{\theta}. \tag{16}$$

Also, since $p = \frac{1}{3}\varphi$, we have

$$\left(\frac{\partial p}{\partial\theta}\right)_v = \tfrac{1}{3}\left(\frac{\partial\varphi}{\partial\theta}\right)_v. \tag{17}$$

Substituting in (15) we have

$$\tfrac{1}{3}\frac{\partial\varphi}{\partial\theta} = \tfrac{4}{3}\frac{\varphi}{\theta}, \tag{18}$$

whence

$$\frac{d\varphi}{\varphi} = 4\,\frac{d\theta}{\theta},$$

and

$$\varphi = C\theta^4, \tag{19}$$

as before.

THE DISTRIBUTION OF MOTION IN A CONDUCTING GAS.

P. G. NUTTING.

WHEN a rarefied gas is conducting an electric current we have every reason to suppose that there is a departure from the uniformly distributed motion assumed by the kinetic theory. The motion of the charged particles constituting the current must be largely longitudinal but, except at very low pressures, this motion must be more or less interfered with by collisions with neutral particles. In any case if we attempt to ascertain the temperature of the gas by inserting in it a flat bolometer strip or thermopile, the temperature which this will acquire will depend upon its orientation with respect to the stream lines. Hence the word temperature applied to a conducting gas is ambiguous if not meaningless unless we at least specify it as longitudinal or transverse

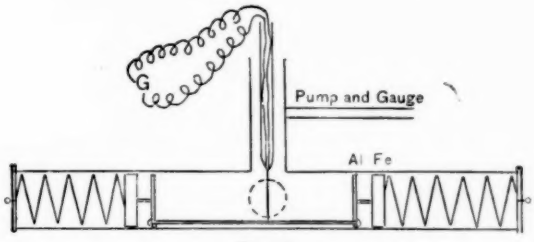


Fig. 1.

meaning by these terms the minimum and maximum temperatures indicated by a flat detector as its orientation is varied. In the present investigation a very thin, wide thermopile was inserted in various parts of the discharge through a wide range of pressures and current densities.

The discharge tube was designed to give as nearly as possible the normal, one dimensional discharge. A straight tube 60 cm. long and 3 cm. in diameter was fitted at its center with a side tube provided with a ground-glass stopper. Through this stopper were in-

serted a glass tube supporting the thermopile and the lead wires to the same. By turning the stopper the pile could be set lengthwise or transversely in the discharge. The ground joint held the lowest vacua without trouble but to turn easily, it required to be occasionally opened and regreased. Both electrodes were of aluminium and were backed with a thick disk of soft iron. They were connected together by a thin glass rod at one edge and to the ends of the tube by spirals of thin copper wire so that the whole system could be moved past the thermopile by means of a magnet. A d'Arsonval galvanometer was used in connection with the thermopile. To measure the current through the tube a Weston millivoltmeter was connected in series. This instrument had a resistance of 8.851 ohms so that the currents quoted are too large by that factor.

Current was supplied by a gang of 500-volt, direct-current dynamos connected in series. Eight, sixteen or twenty-four of these were used but eight of them giving 4,000 volts were found to be ample and more easily controlled. 12,000 volts continuous leaks badly over wood, into the air and across switches. The current was controlled by a cadmium iodide in amyl alcohol fluid resistance.

Thermopiles of several different sizes were used, but all gave practically the same readings under the same conditions, showing that the heating effect was proportional to the area exposed. Fine iron and copper wires were twisted together at the ends, hammered flat, inserted between two disks of copper foil and lightly soldered. The largest pile was about 0.2 mm. thick and 14 mm. diameter. One degree corrresponded to about 2 mm. on the scale. Temperatures of over 100 degrees were recorded with the pile across the tube in a cathode ray discharge.

In calculating the relative heating effects from the temperatures attained, slight losses by convection and conduction were neglected and the law of emission for black bodies, $E = A\,\lambda^{-n}\,e^{-B/\lambda T}$, supposed to hold for the pile. The integral of this is $A\,B^{1-n}\,\Gamma(n-1)\,T^{n-1}$. Since at the stationary temperature the emission and absorption are equal, the ratio of the energy received by the pile in two positions is given by $(T_1^{n-1} - T_0^{n-1}) : (T_2^{n-1} - T_0^{n-1})$. Paschen determined n to be 5.62 for an oxidized copper surface. In most cases however, the temperature changes were so slight that the galvan-

ometer deflections were taken as proportional to the energy received. A specimen set of readings is given below; they were taken at a pressure of 0.063 mm. with the pile in the middle of a cathode dark space 16 mm. long. The subscript $_\perp$ indicates that the pile is across the tube, $_{//}$ that it is placed lengthwise.

i	$S_{//}$	$S_\perp$	S_0	Deflections.			Ratio. $\frac{\Delta T^4}{\Delta T^4}$
				$D_{//}$	$D_\perp$	Ratio.	
5.0	38.03	36.33	39.40	1.37	3.07	2.24	2.57
10.0	36.98	29.28	39.25	2.27	9.97	4.39	5.60
15.0	35.25	26.48	39.02	3.77	12.54	3.33	4.00
20.0	33.60	24.61	38.50	4.90	13.89	2.84	3.36

When the pressure is greater the energy received by the pile in the two positions is more nearly equal, the ratio being only about 1.2 at a pressure of 1 mm. At lower pressures the ratio is greater, for instance at a pressure of 0.015 mm. a ratio of 12 was obtained. In all cases the ratio was found to be a maximum for a moderate value of the current, being much less when the current is either very great or very small.

Outside the dark space in the cathode glow at the same current and pressure, the heating effect with the pile lengthwise in the tube is much greater than in the dark space, while with the pile across the tube it is somewhat less, indicating that the proportion of longitudinal motion is much less. Farther out in the glow the ratio is still less and in the Faraday dark space is practically unity, indicating that here the motion resembles that in a gas not carrying current. In the anode glow the discharge was disturbed by the presence of the pile so that no observations of value were obtained. Connecting the pile and galvanometer to earth appeared to have no effect on the heating of the pile. Charging them positively or negatively with an electric machine appeared to produce no effect, but the galvanometer was so disturbed that the results are uncertain.

As to the mechanism of the discharge, it appears as though the conducting particles were projected normally through the cathode dark space all in nearly paralled paths with about the same velocity. The inert gas appears to be driven back bodily as far as the end of the dark space and the beginning of the cathode glow. Within the

glow, apparently the motion is gradually reduced by collisions to the completely irregular gas motion in the Faraday dark space.

In confirmation of these tentative hypotheses these additional observations may be cited: (1) The width of the dark space is independent of the current strength. Over a range of current from three to seventy the dark space was found not to vary by one per cent. if at all. Increasing the current merely sets more particles in motion with the same velocity. (2) The heating effect on the transverse thermopile in the cathode dark space is directly proportional to the current. (3) Except for small currents and large pressures (in which case it is one-sided) the cathode dark space and cathode glow are symmetrical about a symmetrical cathode, indicating that the charged particles are shot off rather than merely moving along a fall of potential. (4) When by a very slight increase in the current, the discharge is made to change suddenly from the single to the double form, there is no corresponding change in the heating effect. It may be mentioned that it was found impossible at any current or pressure to produce a doubling of the anode discharge as though the positively charged particles were too heavy to be projected off and merely followed the fall of potential. The canal ray glow within the cathode dark space does not extend both ways as though caused by the impact of particles coming from the direction of the anode.

Now the projecting of the changed particles from the electrodes imparts energy to the electrodes themselves, perhaps an amount equal to that given the flying particles. Whether this energy is given the electrodes mechanically or in some unknown way by the mere transfer of the electricity from the metal to the gas particles, it is not easy to decide but the amount of the energy was easily measured. The thermopile was merely made the electrode and earthed. Except at low cathode ray vacua the heating of both anode and cathode was found to be independent of the pressure and directly proportional to the current. Even when the cathode rays were very strong, if they were deflected by a magnet, the heating of the anode was found to be still independent of the pressure and proportional to the current. The heating of anode and cathode were found to be about equal and equal to the heating of

the pile in the normal cathode dark space at the same pressure and current but the results were not sufficiently accurate to establish this relation. The measurement of electrode heating would appear to be a most fruitful method of attacking the problem of gas conduction once the relation of the heating to the nature of the electrodes and the gas used has been determined.

CORNELL UNIVERSITY,
June, 1903.

SOME FURTHER EXPERIMENTS WITH THE COHERER.

BY PHILIP ELY ROBINSON.

THE two experiments herein described were suggested by my previous work [1] on the coherer. In the former experiments a linear receiver was employed, which contained a single contact coherer, and whose total length was 50 cm. The experiments on resonance showed that the half wave-length of this receiver was 49.5 cm. An application of the theory of the Hertzian oscillator to the receiver indicated that the coherer had a capacity of 40 cm.[2] (in electrostatic units). A new receiver of almost double the former length was set up for these experiments. The first investigation consisted in determining its half wave-length, whence the capacity of the new coherer could be compared with that of the former one. Again in the investigation of the coherer with a direct current it was noticed on applying to the coherer ends a potential difference greater than the critical potential difference that the resistance of the coherer seemed to fall instantaneously to a certain value which was termed *impulse value* ("*Sturzwert*")[3] and then to fall slowly further until the relation of the resistance of the coherer to the resistance in the circuit with it was such that the resulting potential difference at the coherer ends had taken on a definite value, which was called the *equilibrium potential difference*. The second investigation is concerned with these impulse values, the object being to determine whether they have a definite value depending upon the applied potential difference, and if so, to see what relation exists between the two. Some work along this line has already been done by Ketterer.[4]

[1] "Der elektrische Widerstand loser Kontakte und seine Anwendung in der drahtlosen Telegraphie," Inaugural-Dissertation. Leipzig: J. A. Barth, 1903. Ann. d. Ph., 11, p. 754, 1903.

[2] Loc. cit., p. 52. The capacity is there given as 41 cm. The difference is due to the employment here of the more rigorous formula (1) on p. 49 instead of the approximate formula on p. 51. Compare footnote on p. 51.

[3] Loc. cit., p. 31.

[4] M. A. Ketterer, J. d. Phys. (4), 1, p. 589, 1902.

1. Resonance.

The receiver used consisted of two straight pieces of copper wire 2.13 mm. in diameter, which carried at one end short steel caps of the same diameter. The total length of each piece including the steel cap was 49.2 cm. The ends of the steel caps were spherical (radius about 2 mm.[1]). These rods were suspended by silk threads from a long glass tube. The steel caps, their spherical surfaces pressing slightly against each other, formed the single contact coherer. Connections with the rest of the circuit were made by short pieces of wire which projected into mercury cups precisely as in the former experiments. Just as formerly a simple Hertzian oscillator was employed. The responsiveness of the coherer or receiver to each wave-length emitted by the oscillator was determined by the resulting resistance of the coherer, a smaller resistance indicating a

Half Wave-lengths. Final Resistances of Coherer in Ohms.

150 cm.	98.4 cm.	97.9 cm.	97.4 cm.	96.9 cm.	75 cm.
1.34	1.50	1.40	1.09	1.40	2.71
2.38	1.37	1.13	1.10	1.37	1.51
2.16	1.29	1.43	1.14	1.54	2.25
1.14	1.00	1.37	1.12	1.51	2.18
1.24	1.16	1.29	1.14	1.33	1.83
2.14	1.58	1.33	.93	1.50	2.75
1.70	1.11	1.33	.88	1.17	2.84
2.15	1.42	1.34	.98	1.37	
1.72	1.07	1.32	1.10	1.31	
1.12	1.29	1.38	1.13	1.50	
Mean 1.71	1.28	1.33	1.06	1.40	2.15

greater responsiveness. The coherer was found to fatigue very quickly, and when fatigued the values of the final resistance for a given wave-length became larger and more irregular. To bring the coherer back to its *fresh* or *sensitive* condition, which was effected by repolishing the contact surfaces, was very difficult. Accordingly numerous determinations of the final resistance for each wave-length were made, and the mean of the ten lowest, (in one case of the seven lowest), was taken as the measure of the responsiveness for that

[1] The former experiments showed that the curvature of the contact surfaces does not influence the capacity nor the critical potential difference of the coherer.

particular wave-length. To show the degree of regularity these determinations are given in full.

Plotting these results as a curve where the ordinates are mean

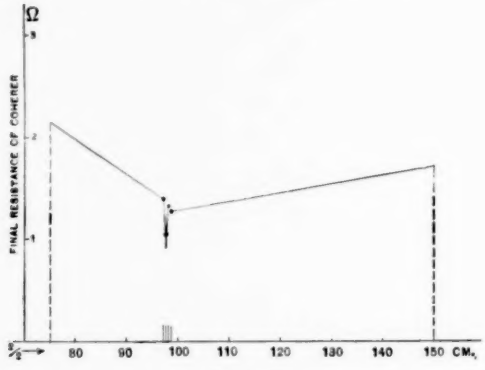


Fig. 1.

final resistances and the abscissas half wave-lengths, the following curve results.

This curve is precisely similar to that found under similar circumstances in the former experiments (compare Curve I., p. 47). The minimum lies at 97.4 cm., that is, the half wave-length of the receiver is 97.4 cm. Applying the formula

$$\frac{2\pi(l_1+\lambda/4)}{\lambda}\,tg\,\frac{2\pi(l_1+\lambda/4)}{\lambda}=\frac{l_1+\lambda/4}{4C\log_e d/R}$$

employed in my former paper (p. 49), and substituting the values:

$$\lambda = 194.8 \text{ cm.}, \quad l_1 = 49.2 \text{ cm.}, \quad d = 98.4 \text{ cm.},$$
$$\text{and} \quad R = 0.106 \text{ mm.},$$

the resulting value of the capacity C of the coherer is 70.4 cm. as against 40 cm. in the previous experiments. Had the capacity been 40 cm. in these experiments, the resulting half wave-length would have been, according to the formula above, 96.7 cm., a result which lies outside the limit of error. On the other hand, had the capacity of the coherer in the former experiments been 70.4 cm., the half wave-length of the receiver would have been 49.7 cm., which lies well within the limit of error there. Consequently one is not able to conclude that the capacity in the two cases was really different.

Further light will be thrown on this point by the experiments of F. Hodson, an account of which will appear shortly.

2. Investigation of Impulse Values.

The method of investigation consisted in applying for an instant to the coherer ends a potential difference greater than the critical potential difference. The resulting resistance of the coherer would then be measured under a low voltage. The method of applying the desired potential difference is indicated in the accompanying figure. The battery E of storage cells of measured voltage was closed through the resistances r_1 and r_2 (usually $r_1 = r_2 = 100\Omega$). From the ends of r_1 wires lead off to the coherer C, one being connected to one end of the latter, while the end A of the other would be dipped for an instant into the mercury cup B which was in connection with the other end of the coherer. In this way the fraction $r_1/(r_1 + r_2)$ of the voltage of the battery E would be applied to the coherer ends, provided the resistance of the coherer was large compared to r_1, and that the effect of the self induction of the circuit be disregarded. The resistance of the coherer was quite large compared to r_1, and the circuit was so arranged that its self induction was small, so that errors due to these causes were neglected. Another arrangement, which it is not necessary to describe here, was employed to measure the resistance of the coherer.

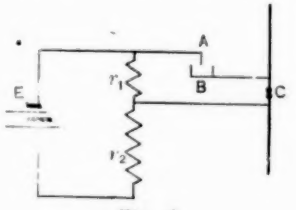


Fig. 2.

The results showed that to each applied potential difference there corresponded a definite final resistance or impulse value. Fatigue of the coherer was very manifest in these experiments, and the difficulty in keeping the coherer at the same sensitiveness prevented me in the limited time at my disposal from getting a series of measurements for the coherer at the same sensitiveness throughout. The results are divided into three sets, which evidently corresponded to different sensitivenesses.

From the first and second columns one or two results which were apparently abnormally low are excluded, and from all columns large results evidently due to fatigue are omitted.

It was noticed in the course of the experiments that the regularity of the results increased markedly as the applied potential difference became greater. Second, that *the greater the applied voltage, the smaller is the impulse value of the coherer resistance.* The relation between the two is evidently not a linear one. Third, the greater the sensitiveness of the coherer the smaller is the impulse value of the coherer resistance for the same applied voltage.

Applied Potential Differences in Volts. Impulsive Values in Ohms.

1.04 *V*	2.12	3.02	1.01	1.55	2.06	2.94	4.25	5.30
52.6 Ω	8.40	6.60	15.2	7.07	3.90	1.62	2.20	1.17
46.3	8.40	5.00	20.4	6.47	4.31	1.44	2.30	1.21
47.8	6.26	4.84	21.2	5.34	3.79	1.50	1.71	
51.7	7.11	6.66	17.2	7.00	4.11	1.56	1.82	
37.6	6.17	7.90		5.51	4.18	1.48	2.20	
50.0	6.19	7.65		6.75	3.96	1.47	2.30	
37.0	8.73	4.37		5.95	3.93	1.52	2.04	
41.1	7.23	5.30		6.37	4.41	1.50	1.90	
53.4	7.25	4.89		6.51	4.32	1.61		
46.1	7.66	6.05		6.65	3.60	1.63		
43.1				5.46	3.70	1.28		
51.3						1.54		
46.5	7.70	5.93	18.5	6.28	4.02	1.51	2.06	1.19

Mean values.

If, as I believe, the value to which the coherer resistance falls when acted upon by electric waves corresponds to an impulse value, then by comparing the value produced by electric waves with the impulse values, we should be able to measure the potential difference initially set up at the coherer ends by the electric waves, provided the sensitiveness of the coherer be the same in both cases. Thus comparing the impulse values given above with the values of the final resistances given in the first part of this paper, we are lead to conclude that in the experiments on resonance the waves from the 5 m. distant oscillator set up at the coherer ends a potential difference which in every case was greater than 2 volts and which probably was over 3 volts.

In my former experiments fatigue of the coherer had only been called forth by electric radiations. These experiments showed that fatigue can also be called forth by a direct current. The ap-

plied voltages were such that it was not impossible for a spark discharge to occur between the coherer ends. The suggestion offered in my former paper that the fatigue may be caused by such a spark, which would increase the density of the layer of oxide on the contact surfaces of the coherer, would then still be applicable. This subject of fatigue I hope shortly to investigate further.

In conclusion I wish to thank Professor Michelson for placing the facilities of the Ryerson Physical Laboratory at my disposal.

UNIVERSITY OF CHICAGO,
June 3, 1903.

MAGNETIC DOUBLE REFRACTION.[1]

BY ARTHUR W. EWELL.

DR. QUIRINO MAJORANA[2] has shown that certain specimens of dialyzed iron (a colloidal solution of peroxide of iron) become double refracting in a magnetic field, the optic axis having the direction of the magnetic lines of force.

It seemed to the author of interest to repeat Majorana's experiments with such specimens of dialyzed iron as could be obtained locally, to use an alternating magnetic field and to test other substances with a very sensitive apparatus for detecting double refraction, suggested by Lord Rayleigh.[2]

A magnet suitable for alternating currents was constructed by replacing the laminations of one of the shorter sides of the core of a five-kilowatt, core-type transformer, by laminations cut to give an air gap of 0.5 cm. between faces 1 cm. wide and 12.5 cm. long, outside of which the laminations were cut obliquely to concentrate the field. The core was wound with 86 turns of No. 2 cable which was calculated to give the maximum field with an alternating current of 60 amperes at 220 volts. The magnet was calibrated for direct currents by withdrawing from between the poles an exploring coil connected with a ballistic galvanometer with which it had been calibrated and for alternating currents, from the E.M.F., measured with a low reading a. c. voltmeter, induced in the same coil in the air gap.

[1] When this article was in type I became acquainted with the second paper by August Schmauss in No. 9 of this year's volume of Annalen der Physik, in which he describes the temperature reversal and more fully than I the behavior of glycerine and jelly solutions, and plausibly develops the theory that the phenomena are due to the solutions consisting of minute particles in suspension in a medium of different and differently varying permeability.

[2] Academia dei Lincei, Rendiconti; 1902, X., pp. 374, 463, 531, XI., pp. 90, 131. Phil. Mag., 1903, April, p. 486. C. R., 1902, pp. 159, 235.

Theoretical discussion by W. Voigt (who sought in vain for double refraction in heavy flint glass) in Ann. der Physik, 1902, VIII., p. 880.

Brief note by A. Schmauss in Ann. der Physik, 1903, III., p. 658.

[3] Phil. Mag., 1902, Dec., p. 678.

The liquid tested was contained in a trough with plate-glass ends which fitted into the horizontal air gap. The source of light was a three-glower 150 c. p. Nernst lamp, with the filaments vertical and the plane of the filaments oblique to give the equivalent of a very brilliant band of light of nearly the width of the air gap. Between the lamp and the magnet was a Nicol set at 45° to the magnetic field. On the other side of the magnet was a horizontal strained glass plate, in front of which was a Nicol, crossed with reference to the first, and finally a short-focus telescope. The strained glass plate was 0.28 cm. thick, 30 cm. long and 3.3 cm. broad. It rested edgewise on supports, near the middle, 3 cm. apart and the ends were pulled down by rubber bands towards the base on which these supports rested. The horizontal dark band, the neutral axis of the bent glass plate, was viewed through the telescope and its location fixed by two silk fibers attached to the glass strip parallel to the edges of the band. If any appreciable double refraction appeared in the air gap the dark band was displaced to a position where this double refraction was neutralized. The sensitiveness of the apparatus was determined from the observation that 100 g. pressure upon a piece of plate glass 2.5 cm. wide gave an appreciable upward movement of the band. According to Mascart,[1] for average crown glass, 10 kilos pressure per mm. width produces a difference of path of 0.5 λ. (This value was adopted by Lord Rayleigh and agrees well with the recent results of F. Pockels.[2]) I could, therefore, detect a difference of path of 0.0002 λ. The minimum difference which Dr. Majorana could detect was 0.004 λ. The greater sensitiveness of the optical apparatus much more than compensates for the weaker magnet, the maximum field which I could obtain being 7,000 units while Dr. Majorana's was 18,000. The length of his air gap was 7 cm. Crown glass, subjected to pressure behaves as a negative crystal. As the magnetic lines of force were horizontal an upward movement of the band indicated positive magnetic double refraction.

A large number of drug stores in Worcester, Boston, Lynn, Salem and other places in Massachusetts were visited or commun-

[1] Optique, t. II., p. 232.

[2] Ann. der Physik, 1902, VII., p. 745.

acted with in a search for very old specimens of dialyzed iron and a number of such specimens were obtained. Several specimens of fresh product and also of intermediate age were also secured. The older specimens gave enormous motion of the dark band, generally negative. Specimens fresh from the preparers gave positive double refraction of a few thousandths λ. Dialyzed iron carefully prepared according to the directions of Dr. Majorana showed no double refraction after ten days' dialysis. Majorana states that his own preparations were uncertain and never very active. Ferrous and ferric chloride gave slight positive double refraction.

The following substances in the densest solutions consistent with sufficient transparency, gave no double refraction : ferrous sulphate, ferric sulphate, iron alum, citrate of iron, tincture of dialyzed iron, eisen zücker, nickel chloride, nitrate and carbonate, colloidal nickel, cobalt sulphate, chloride and nitrate, manganous sulphate and chloride. Water and carbon bisulphide were also inactive.

For quantitative study of the dialyzed iron, the strained glass plate was replaced by a Babinet compensator. Unless otherwise stated the light from the Nernst lamp was filtered through several thicknesses of red glass, giving quite monochromatic light of measured wave-length 6,600 Ångström units. The movement of the wedge to introduce a difference of path of one wave-length of this red light was 15.2 cm.

The double refraction of a specimen of dialyzed iron was found quite definite for a given age, concentration, magnetic field and temperature as is illustrated by the following observations upon a dilution of what will be designated specimen F (over 8 years old), of concentration 1:40 (1 c.c. of dialyzed iron to 40 c.c. of water, density equal 1.0033). The figures for the movement of the band are the means of three independent (usually identical) readings and are all negative.

Amperes,	28	33	53	65	86
Movement of band,	1.2	3.2	5	6	7.5

Two days later, a different sample of the same solution.

Amperes,	36	60	80	23	50	91
Movement of band,	3	6.7	7.8	1.1	5.8	7.2

Freshly prepared dilution of same concentration.

Amperes,	38	61	16	49	70
Movement of band,	3	6.4	8.4	4.9	6.6

Another freshly prepared dilution upon the following day.

Amperes,	15	24	34	48	54	66	76
Movement of band,	.2	1	2.4	4.7	6	6.9	7.7

These results are plotted in Fig. 1, I., with the strength of field as abscissæ, determined from the current and calibration curve, and as ordinates, the difference in path, in terms of the wave-length of the red light, obtained by dividing the movement of the bands by 15.2.

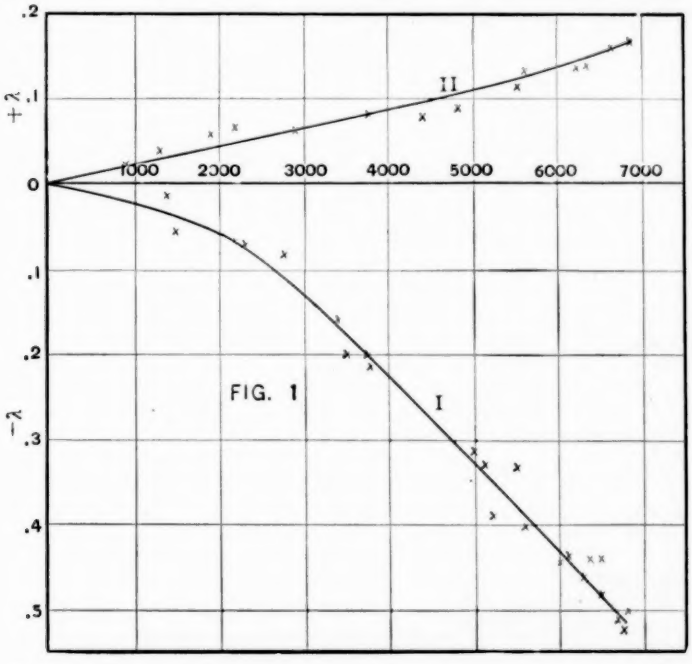


FIG. 1

In Fig. 1, II are plotted observations upon a 1 : 20 dilution of specimen B which was above five years old and which gave the greatest positive double refraction.

Figs. 2 and 3 illustrate the effect of varying the concentration. Fig. 2 represents observations upon specimen M which was at least seven years old and probably much older. The four concentrations were: I., 1 : 160; II., 1 : 80; III., 1 : 40; IV., 1 : 20. Fig. 3 gives similar observations upon specimen F of concentration: I., 1 : 80; II.

1:40; III., 1:20.[1] Three other specimens gave very similar results. On plotting the logarithms of these observations, curves are obtained which are very nearly straight lines inclined at an angle whose tangent is 2, indicating that *the double refraction is proportional to the square of the strength of field.* Figs. 2 and 3 show

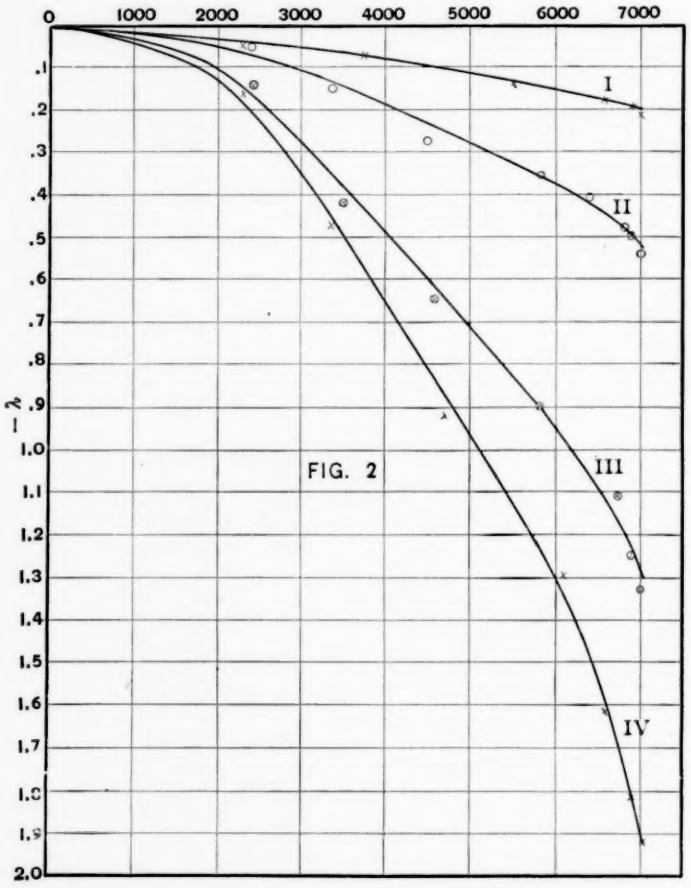


FIG. 2

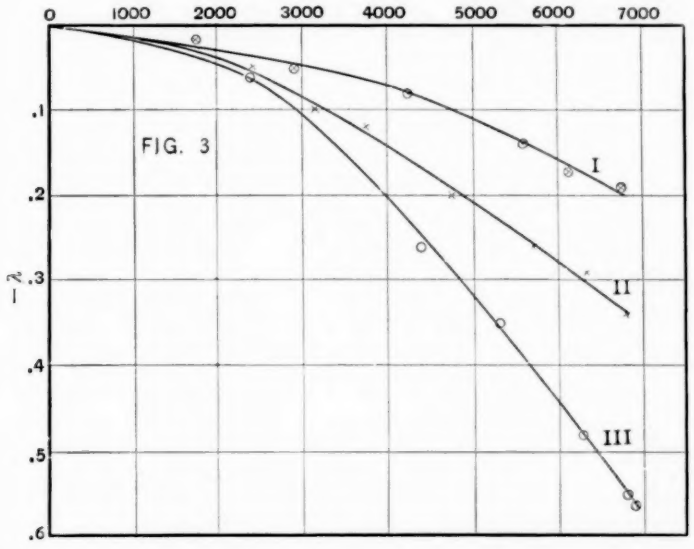
also that *the double refraction is proportional to the concentration for weak concentrations but for denser solutions does not increase in proportion to the concentration.*

Dr. Majorana concluded that the double refraction was proportional to the concentration but gives none of his observations. He also found the double refraction approximately proportional to the

[1] Fig. 3 represents observations at a higher temperature than Fig. 1.

square of the field, extending his observations to the limit of his more powerful magnet.

Dr. Majorana found but two unstable specimens which gave negative double refraction in weak fields. The most active specimens which he studied were positive in weak fields, reversed as the field



increased and for intense fields were strongly negative. I could detect no such reversal in weak fields. Even in fields so weak as to give a barely perceptible movement of the dark band in the Rayleigh apparatus the double refraction was still negative.

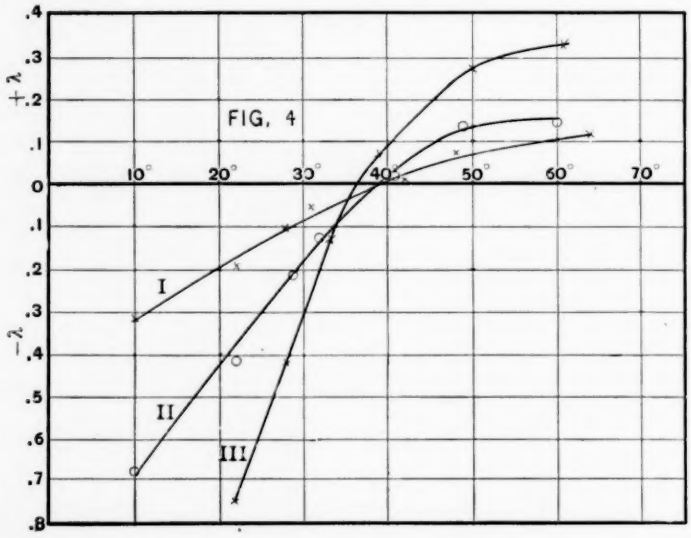
Specimen.	Field.	$(d.p.)_r$	$(d.p.)_g$	$\frac{(d.p.)_g}{(d.p.)_r}$
M 1 : 160	6,800	.27	.38	1.41
"	5,700	.20	.29	1.45
M 1 : 180	6,800	.48	.64	1.34
"	5,700	.37	.53	1.43
T 1 : 80	6,800	.31	.47	1.52
"	5,700	.24	.35	1.46
C 1 : 80	7,000	.13	.21	1.56

To determine the dispersion of the double refraction observations were made both with the red light and also with a green light of mean measured wave-length 5,300, obtained by filtering the light

through a solution of potassium chromate and copper chloride. The iron solution absorbed all light beyond the green. The movement of the wedge corresponding to a difference of path of one wave-length was 12.9 mm. The final means of a number of observations upon the difference of path for the two wave-lengths are given below.

The quotient of the wave-lengths is 1.22 and the square of this quotient 1.48 which would roughly indicate that *the double refraction is inversely proportional to the square of the wave-length* which was Dr. Majorana's conclusion.

Upon applying alternating currents, for which the magnet was primarily constructed, it was observed that while the double refraction appeared at first, the position of the dark band was less definite



owing to its periodically varying position with the rising and falling field and that with the negatively active solutions, the activity decreased with prolonged application of the current and finally became zero. The cause of this second characteristic was soon found to be the heating of the solution owing to hysteresis in the iron of the magnet and thus being led to a study of the dependence upon temperature, the remarkable fact was discovered that *all these negatively active solutions gradually lose their negative activity as the temperature is raised until it becomes zero at about 40°, when, if*

the temperature is still further raised, they acquire positive activity. Upon cooling, negative activity returns of the same magnitude as before heating.

Fig. 4 gives observations upon three concentrations of specimen *F* in a field of 6,800 units: I., 1:80; II., 1:40; III., 1:20. Fig. 5 represents observations upon a 1:80 dilution of specimen *T* which was at least five years old; I., red light, field = 5,700; II., red light, field = 6,800; III., green light, field = 6,800. Fig. 6, I., II. and III., are similar observations upon a 1:80 dilution of specimen *M*; I., red light, field = 5,700; II., red light, field = 6,800; III., green light, field = 6,800. Accurate readings could not be made at higher temperatures but qualitative observations indicated that near the boiling point the double refraction remained positive of

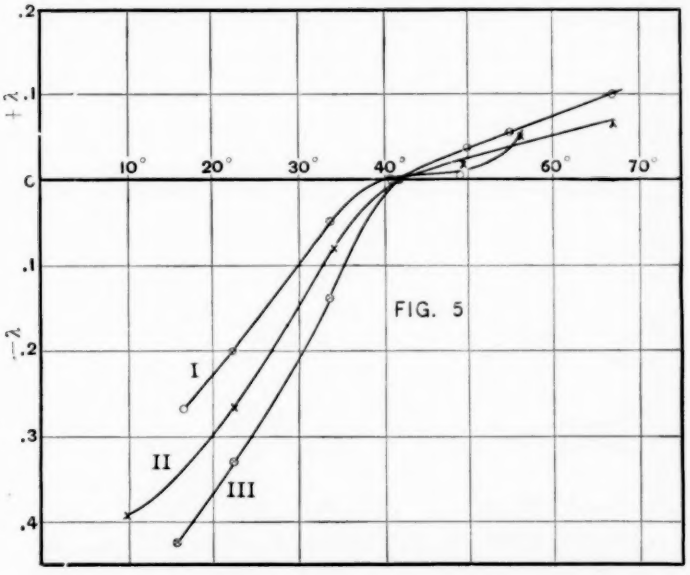


FIG. 5

about the same magnitude as at the highest temperatures represented in the figures. All the negative specimens, five in number, behaved similarly.

Positively active specimens showed simply a continuous decrease in activity with rise in temperature. Observations upon a 1:20 dilution of specimen B in a field of 6,800 units are given in Fig. 6, IV.

The Less Mechanical Freedom in a Solution the Less the Activity.

A 1 : 40 solution of F in glycerine was but slightly over half as active as a solution of similar concentration in water. 1 c.c. of F was dissolved in melted jelly composed of 2 grams of calves' foot gelatine to 10 c.c. of water to 10 c.c. of glycerine. As the jelly solidified the activity, measured at intervals in a direct field, decreased, becoming when the jelly was firm but 0.1 λ for a length of 2.9 cm.

A mixture of the positive B and strongly negative F might be either positive or negative according to the proportion. A mixture in the proportion of 10 : 1 was inactive.

Both positive and negative specimens were found paramagnetic by Quincke's manometric method.

Dr. Majorana also discovered magnetic dichroism, *i. e.*, an

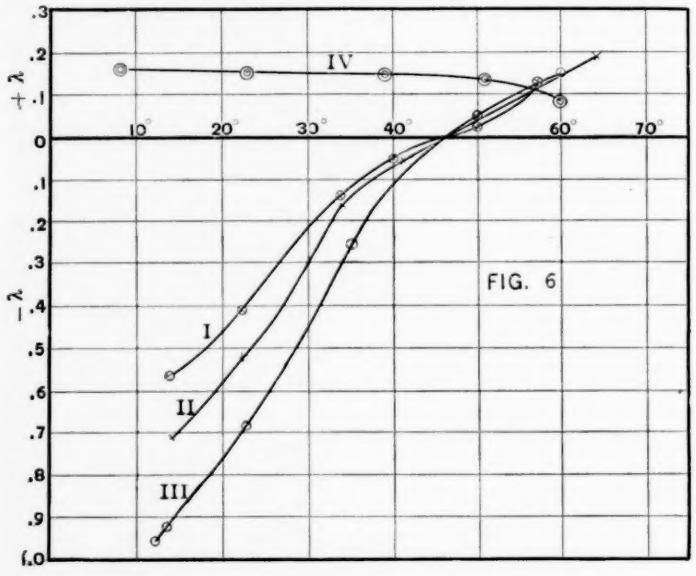


FIG. 6

unequal absorption of the light components parallel and perpendicular to the field, resulting in certain cases in a rotation of the plane of polarization. The author observed no such effect except in one specimen where there was a slight increased absorption for light polarized perpendicular to the field. Majorana found this dichroism conspicuous only in specimens which showed reversal in weak fields which was not characteristic of any of my specimens.

Dr. Majorana gives two methods for distinguishing active from

inactive specimens: 1°, from the appearance in reflected light, the active having a turbid appearance. Some of my active specimens had this appearance and some did not and the same was true of inactive specimens. 2°, from the solubility of the precipitate upon adding nitric acid, the greater the insolubility the greater the activity. This rule applied better to my specimens, all those giving a readily soluble precipitate being little active, but when the precipitate was not readily soluble the insolubility was not uniformly proportional to the activity. This partial chemical difference suggested that the active solutions might be St. Gilles' modification of soluble iron oxide, but on preparing the latter it was found inactive.

In general the older a specimen, the greater the activity which was negative in the oldest specimens. Dr. Majorana considers that ferric chloride is eliminated by age. In confirmation, the author found that one drop of concentrated ferric chloride solution in 15 c.c. of F, 1 : 40, reduced the activity 30 per cent.

I wish to thank Prof. H. B. Smith for very kindly placing at my disposal the extensive facilities of the Electrical Engineering Department.

WORCESTER POLYTECHNIC INSTITUTE,
August 1, 1903.

ENERGY IN THE VISIBLE SPECTRUM OF THE HEFNER STANDARD.

BY KNUT ÅNGSTRÖM.

I. INTRODUCTION.

IT is known that the sensitiveness of the eye to light varies with the different wave-lengths and that it is possible to compare with one another only the intensities of sources of light having the same color. We are reduced therefore in photometric researches to the comparison of lights of similar color or indeed when it is a question of spectrophotometric work of intensities in the same spectral region. This gives us, however, no idea of the distribution of energy of radiation in the spectrum since no source of light suitable to serve as a standard has been studied with respect to its distribution of energy. Unfortunately the intensity of radiation in the visible spectrum of our photometric standards is so feeble that although the problem of the distribution of energy in the spectrum has been under consideration for a long time, such a determination has never been made. Nevertheless in the last few years results of the highest importance have been obtained. I shall recall here only the important experimental researches upon the relation between temperature and wave-lengths made by Paschen, Lummer and Pringsheim as well as the no less admirable theoretical developments of Wien, Planck, Lorentz and others. For the infra-red spectrum, in which the intensity of radiation is relatively great, the formulæ established for expressing the relation of the energy to the temperature and to the wave-length have been rigorously tested, but the experimental methods employed thus far have been insufficient for the study of the visible spectrum.

It is, however, of double interest to extend the investigation of spectral energy to the luminous spectrum. It is only by such researches that we shall be able to determine by photometric measurements the distribution of energy in that part of the spectrum. In

the second place it will then be possible to decide in how far the formulæ established for the radiation of a black body are applicable to the visible spectrum of our ordinary sources in which the light is derived from the radiation of solids.

The object of the present experiments is to contribute to the solution of these questions. The Hefner lamp lends itself well to practical photometry and it is the distribution of energy in this lamp that I have sought to determine. I have attempted also to find the total energy of a radiation of the "bougie-metre" (lux) and to give the energy of its spectral radiation in absolute measure. As a secondary result I have obtained the corresponding values for an electric glow lamp.

Julius Thomsen[1] attempted as early as 1865 to determine the energy of luminous radiation or as it has also been called the mechanical equivalent of light and in 1889 Tumlirz[2] determined the same quantity for the Hefner lamp. He determined the total radiation Q in absolute measure by means of a sort of air thermometer and the luminous radiation L by suppressing the infra-red radiation by absorption in a thick layer of water. This method of separating the luminous radiation from the infra-red, a method which has been employed even in recent times leaves as one may readily comprehend much to be desired, the ratio L/Q between the luminous radiation and the total radiation thus found being too large. The value of L/Q will moreover always to a certain extent remain an arbitrary matter since it depends upon the choice of the boundary between the visible spectrum and the infra-red spectrum. In what follows we shall fix this limit as is commonly done at $\lambda = 7{,}600$ (Ångström units) or $\lambda = 0.76\ \mu$.

The Hefner lamp employed is a normal lamp purchased from Siemens and Halske. It is of the form adopted by the Physical Technical Institute at Charlottenburg and is furnished with an arrangement for adjusting the height of the flame. An auxiliary attachment permits of exact control of the different parts of the lamp. The exterior diameter of the tube is 0.82 cm.; the interior diameter 0.8 cm.; its height above the reservoir of the lamp is 3 cm.; and the height of the flame 4 cm. The amyl acetate em-

[1] Thomsen, J., Pogg. Ann., 125, 348, 1865.
[2] Tumlirz, O., Wied. Ann., 38, 640, 1889.

ployed was furnished by the Pharmacy of the University of Upsala. The radiation from this lamp falling upon a surface of 1 sq. cm., at a distance of one meter, constitutes the unit of illumination or the bougie-metre (lux) examined here.

II. General Character of the Radiation of the Hefner Lamp.

The energy spectra of flames arising from the combustion of the various hydrocarbons all possess essentially the same character. Upon the continuous spectrum of the incandescent particles of carbon are superimposed the spectra of the gaseous products of combustion; that is to say the discontinuous spectra of water vapor and of carbon dioxide. The radiation of the Hefner lamp offers no exception in this respect.

To obtain an idea of the general character of the infra-red spectrum of the Hefner lamp I have made use of the spectro-bolometer with photographic recording device described elsewhere.[1] With this instrument I have recorded the energy of the spectrum of an incandescent lamp; I have compared the same with the continuous spectrum of the Hefner lamp and I have sought to determine the current in the lamp for which the two energy spectra are as nearly identical as possible. The electric lamp employed for this purpose and in the researches of which I shall give account in what follows came from the Bavarian manufactory of glow lamps in Munich. The filament of this lamp is very thin and is maintained perfectly rigid by another heavier strip of carbon in the form of an arc. Under normal conditions this lamp requires a tension of 16 v. and a current of 0.25 amp. I have replaced the glass globe of the lamp by a tube made for the purpose which is shown in Fig. 1. A plate of fluorspar is hermetically cemented to the opening A, the tube B contains phosphorus anhydride and the filament is surrounded by a cylindrical tube of metal blackened on the interior by means of platinum and furnished on the side turned towards the window with a small rectangular opening.

Fig. 1.

[1] K. Ångström, Acta. Soc. Upsal., 1895; PHYS. REV., III., 137, 1895.

The lamp was supplied by current from eight large accumulator cells. The current was regulated by means of resistance in the circuit and was determined by means of an accurate ammeter. By means of a concave mirror of focal length 67 cm. an image of the source of light was projected upon the face of the bolometer. In Fig. 2 the following curves are shown : *a*, curve for the radiation of

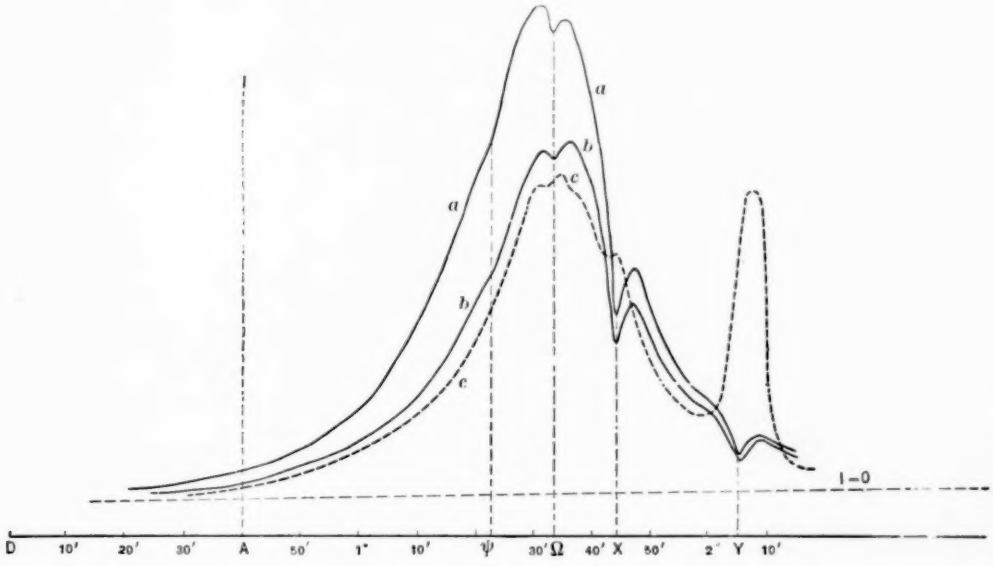


Fig. 2.

the glow lamp with a current of 0.2 amp. ; *b*, curve for the glow lamp with a current of 0.21 amp. ; *c*, curve for the Hefner lamp.

From the beginning of the infra-red spectrum to about $\lambda = 1.5\ \mu$ the energy spectrum of the Hefner lamp is not affected by emission bands. In this region the spectrum corresponds very well with that of the glow lamp for a current of 0.21 amp., as will be seen from the following table in which are given the mean values of the intensity I_1, obtained by measuring three photographic plates of the spectrum of the glow lamp and of I_1, from five plates of the Hefner lamp.

The spectral distribution being identical in the two cases, I/I_1 should be a constant, and this will be seen to be the case from the numbers given in the fifth colum of the table.

TABLE I.

Dev.	λ	I	I_1	$\frac{I}{I_1}$
1°	0.90	6.9	7.9	0.875
1°5′	0.96	9.1	10.5	0.850
1°10′	1.06	12.2	14.5	0.844
1°15′	1.18	17.0	19.9	0.854
1°20′	1.33	22.8	27.5	0.830
1°25′	1.50	29.9	35.3	0.846

III. DISTRIBUTION OF ENERGY IN THE VISIBLE SPECTRUM OF THE GLOW LAMP AND OF THE HEFNER LAMP.

To determine the distribution of energy in the visible spectrum I have employed the arrangement of apparatus shown in Fig. 3, in which *MPM* is a reflecting spectroscope with silvered mirrors. As a source of radiation I have used the glow lamp described above.

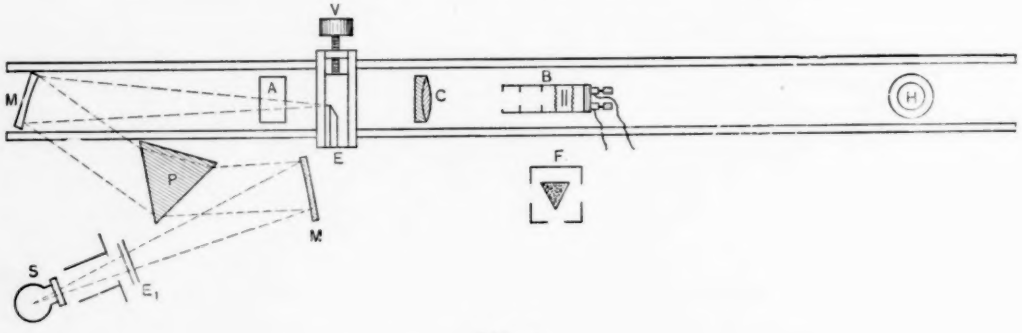


Fig. 3.

The filament being very slender it was not necessary to furnish the spectroscope with a slit in order to obtain a sufficiently pure spectrum. By this apparatus a real image of the spectrum is produced of which a portion may be intercepted by a double screen E, movable perpendicularly to the bundle of rays by means of a micrometric screw V. The rays which pass the edge of the screen are concentrated at B in an image of uniform color by means of the cylindrical lens C. At B may be placed a bolometer or a photometer F. To avoid the disturbing influence of the radiation of heated portions of the instrument a water cell is placed at A. A screen E_1 is used to cut off at will the radiation emitted by the

source S. All parts of the apparatus are mounted upon a very solid optical bench which permits of ready adjustment.

If E is withdrawn so that the whole of the visible spectrum passes it is evident that the image at B will be white. Placing at H a Hefner lamp and substituting a photometer F for the bolometer B the current in the glow lamp S may be so regulated that the light of this lamp at B will have the same color as that coming from the Hefner lamp. The current necessary was found to be 0.21 amp. and by spectrophotometric comparisons of the two lights under these conditions the distribution of energy in the spectra was found to be same; which agrees perfectly with the results already obtained in the infra-red spectrum.

The photometer is now replaced by a spectroscope furnished with a scale the reading of which in wave-lengths has been determined experimentally. The screen E is advanced gradually and for each adjustment of the micrometer screw the limiting wave-length of the light which passes is determined. This done the spectroscope is replaced by a very sensitive bolometer and the radiation is determined for different positions of the screen E. In this way the total energy of that portion of the spectrum between the extreme ultra-violet is measured and a certain wave-length determined by the position of the screen; that is to say $\int_0^\lambda I_\lambda d\lambda$. These values are designated by L and L_λ.

The radiation which passes the edge of the screen and which produces the effect upon the bolometer is evidently not altogether pure; it is mixed with stray light, that is to say with light that ought to have been intercepted with the screen. To correct the errors caused by this stray light I have proceeded as follows:

The spectroscope, furnished with a double Vierordt slit and with a rectangular diaphragm in the focal plane of the eye-piece, was mounted in such a position that the opening of the slit was at B, Fig. 3, so that half of this slit received the light from the glow lamp and the other half that from the Hefner lamp which served as a source of comparison. The intensity of the light in various parts of the spectrum, the screen E being completely withdrawn, was then determined; after which the intensity of the same regions

was found for different positions of the screen. These measurements give the ratio between the stray light and the light in the original spectrum for various wave-lengths and positions of the screen. By introducing the values found for L as ordinates and wave-lengths as abscissæ, a curve is obtained by means of which one may, with sufficient exactitude for our purpose, construct geometrically the ordinary curve for the intensity I; for which purpose we make use of the fact that the tangent of the first curve $(dL/d\lambda)$ gives I directly. Let abc (Fig. 4) be the curve thus found

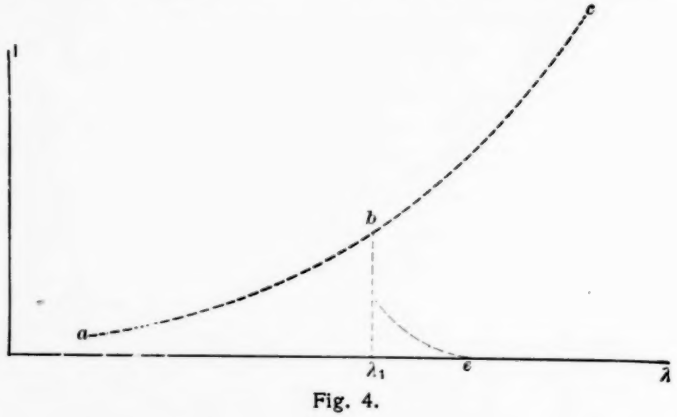


Fig. 4.

and let the screen E intercept the spectrum at λ_1. By aid of the photometric determinations described above we can trace the curve representing the intensity of the stray light in the shaded region $\lambda_1\ \lambda$. The ratio between the surface $be\lambda_1$ and $ab\lambda_1$ evidently gives the correction for the stray light; a correction which it is necessary to subtract from the original value of L. The correction was very insignificant, being for the following positions of the screen:

$\lambda = 0.70\,\mu$ 1.2%
$\lambda = 0.65\,\mu$ 1.4%
$\lambda = 0.60\,\mu$ 1.4%.

Omitting the numerous determinations of a preliminary nature I give in the following tables, in the columns marked L_1, some of the best sets of observations obtained; the values being corrected for stray light. Each value of L_1 is the mean of from six to ten determinations.

IV. Determination of the Radiation of the Hefner Lamp in Absolute Measure.

To reduce the relative values to absolute measure I have first determined the total radiation Q of the Hefner lamp by means of my pyrheliometer with electric compensation [1] and then the ratio $L_{0.76}/Q$ of the luminous radiation to the total radiation. I have thus obtained $L_{0.76}$; after which it is easy to transform the other values of L into absolute measure.[2]

(*a*) *Determination of the Total Radiation.*

The pyrheliometer employed in this experiment was a very good instrument with all the refinements due to experience. The strips for absorbing the heat were of manganin and of a width of 0.1497 cm., the resistance being 0.0915 ohms per cm. From these values I have been able to determine the constant of the instrument to be 0.1314 and I have computed the intensity of the radiation from the value of the electric current used in compensation by means of the equation

$$Q = ki^2 \frac{\text{gr. cal.}}{\text{sec.} \times \text{cm.}^2}.$$

The current i was determined by means of an accurate amperemeter from Siemens and Halske. The equality of temperature of the two strips was indicated by a mirror galvanometer of great sensitiveness. The determinations have been made at two different distances from the lamp respectively 100 cm. and 50 cm. The results for a distance of 100 cm. are as follows:

Right side of pyrheliometer illuminated, $i = 0.0127$ amp.
Left " " " " $i = 0.0128$ "

From which we have

$$Q_{100} = 0.1314\ (0.01275)^2 = 0.0000214 \frac{\text{gr. cal.}}{\text{sec. cm.}^2}.$$

For a distance of 50 cm. we obtain in the same way:
Right side of pyrheliometer illuminated, $i = 0.0253$ amp.
Left " " " " $i = 0.0259$ "

[1] K. Ångström, Acta. Soc. Upsal., 1893; Phys. Rev., I., 365, 1893.
[2] See my preliminary note: K. Ångström, Phys. Zeit., Vol. 3, 257, 1902.

Whence

$$Q_{50} = 0.1314\ (0.0256)^2 = 0.000086\ \frac{\text{gr. cal.}}{\text{sec. cm.}^2}.$$

If from the latter determination we calculate the radiation for a distance of 100 cm. we get (evidently)

$$Q_{100} = 0.0000215\ \frac{\text{gr. cal.}}{\text{sec. cm.}^2};$$

which is in good agreement with the preceding value. We have thus determined the total radiation corresponding to the *bougie-metre*.

(*b*) *Determination of the Photogenic Efficiency* L/Q.

For the determination of the ratio between the luminous radiation and the total radiation the arrangement which has been described above (Section II.) was used. The screen E was adjusted for $\lambda = 0.76\,\mu$ so as to intercept only the infra-red spectrum. The photometer F was placed at B and a Hefner lamp (see Fig. 3) was placed at H. The photometer (Fig. 3, F) consisted of a rectangular prism of chalk enclosed in a small black box with three circular openings, two of which were located face to face for the passage of the light to be examined, while the third was so placed as to enable the observation of the illumination of the two faces of the prism. The Hefner lamp was adjusted so that the light upon the two sides of the photometer was uniform. In this case the photometer receives on one side the total radiation from the Hefner lamp, on the other side only the luminous portion. By replacing the photometer with a sensitive bolometer for the purpose of measuring the intensity of these two bundles of rays, which are physiologically identical, we obtain the value L/Q.

I have obtained as the mean of four determinations on different occasions $L/Q = 0.0096$.

This determination is a very difficult one, the least variation in the adjustment of the screen causing large errors in the result. I fear that the accuracy of the value of L/Q given above is not within four per cent. Q and L/Q being known, we may deduce L for $\lambda = 0.76$. Thus we obtain

$$L_{0.76} = 0.0096 \times 0.0000215$$
$$= 20.6 \times 10^{-8}\ \frac{\text{gr. cal.}}{\text{sec. cm.}^2}.$$

It is from this value of $L_{0.76}$ that the values of $L \times 10^8$ given in Tables II. and III. have been deduced.

V. Radiation in the Luminous Spectrum of the Hefner Lamp Expressed as a Function of the Wave-Length.

It may be taken as probable, *a priori*, that the spectral distribution of the energy of radiation both in the incandescent lamp and in the Hefner lamp may, by a suitable choice of constants, be expressed by means of formulæ given for the radiation of an absolutely black body. For the visible spectrum, where the wave-lengths are relatively small, Planck's formula

$$I = C_1 \lambda^{-5} (e^{c_2/\lambda T} - 1)^{-1} \tag{1}$$

and that of Wien

$$I = C_1 \lambda^{-5} e^{-c_2/\lambda T} \tag{2}$$

lead to the same result in practice. The latter being the more simple I have attempted to determine for it the constants C_1 and c_2 in such a manner that the formula shall represent the observations as well as possible. It is not however the intensity I which has been the object of our observations, but

$$L_\lambda = \int_0^\lambda I_\lambda d\lambda = C_1 \int_0^\lambda \lambda^{-5} e^{-c_2/\lambda T} d\lambda. \tag{3}$$

The temperature T being taken as constant in our sources of light, we may replace C_2 by a constant c and obtain through integration the expression

$$L_\lambda = C_1 \frac{e^{-c/\lambda}}{c^4} \left\{ \left(\frac{c}{\lambda}\right)^3 + 3\left(\frac{c}{\lambda}\right)^2 + 6\left(\frac{c}{\lambda}\right)^3 + 6 \right\}. \tag{4}$$

From the directly observed values of L, values have been obtained by interpolation of the means for $\lambda = 0.50$, $\lambda = 0.55$, $\lambda = 0.60$, $\lambda = 0.65$ and $\lambda = 0.75$. By the method of least squares I have deduced from these values the constants C_1 and c by the aid of approximate values. This computation gives as a result, $C = 0.0160$, $c = 7.85$.

The following table contains the observed values and the calculated values with their differences.

TABLE IV.

λ	$L \times 10^8$		Obs. — Calc.	$I \times 10^7$
	Obs.	Calc.		
0.75	18.7	18.46	+0.24	19.26
0.70	10.4	10.67	−0.27	12.83
0.65	5.44	5.46	−0.02	7.85
0.60	2.61	2.51	−0.10	4.28
0.55	1.17	0.97	+0.20	2.02
0.50	0.34	0.30	+0.04	0.78

It will be seen that the difference between the computed and the observed values does not appear to pass the limits of the errors of observation. Consequently one may, with the degree of exactitude which the experiments permit, express the determination of the spectrum of the Hefner lamp by the formula

$$I_\lambda = 0.0160\lambda^{-5}e^{-\frac{7.85}{\lambda}}. \tag{5}$$

The last column of the preceding table contains the values of I calculated according to this formula. The formulæ 4 and 5 are graphically represented in Fig. 5, the former by means of the curve marked I, the latter by means of that marked L. The first gives directly the energy between two wave-lengths λ_1 and λ_2 by means of the differences between the corresponding ordinates. The curve I is an ordinary curve for intensities.

Paschen and Wanner[1] have determined the constant c_2 in formula 2 for the radiation of a black body and have found $C_2 = 14{,}440$. Since in formula (5) we have $c = 7.85 = C_2/T$, we may determine the absolute temperature of a black body emitting radiation equal to that of the Hefner lamp. We obtain for this the value $T = 1{,}830°$.

This value is in good agreement with the determination of Lummer and Pringsheim[2] who by use of Paschen's law,

$$\lambda T_{\max.} = \text{const.},$$

[1] Paschen and Wanner, Sitzungsber. d. k. Akad. de W. Berlin, 1899.

[2] Lummer & Pringsheim, Verhandl. d. Deutsch. Phys. Ges., III., p. 37, 1901.

have determined T for various sources of radiation under the supposition that the constants are comprised between 2.940 and 2.630, values determined respectively for the radiation from a black body and from platinum. For a candle, the conditions of radiation of which appear to approach most nearly that of the Hefner lamp, they

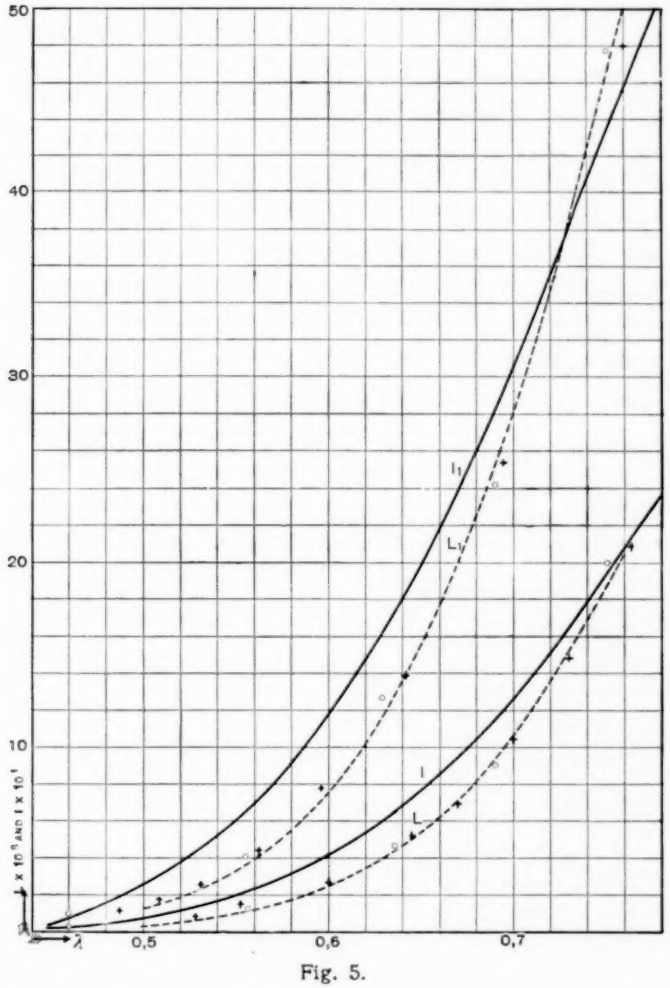


Fig. 5.

have obtained $T_{max.} = 1.960°$ $T_{min.} = 1.750°$. I also sought to determine the constant c for the incandescent lamp when the current in the circuit was 0.253 amp. I found $c = 7.24$ (taking $C_2 = 0.0160$ as in the previous calculation). The corresponding curves L_1 and I_1

are also represented in Fig. 5 with the observed values so that it is possible to compare the observed and calculated values. The temperature in this case corresponds to 2,000°.

Our investigations show that Wien's law represents very well the distribution of energy in the luminous spectrum of the Hefner lamp and of the incandescent lamp. After having thus determined in absolute measure the radiation and the constants for the Hefner lamp in Wien's formula, it is easy to measure photometrically the energy of radiation and its distribution in the visible spectrum of any radiant source whatever, with an exactitude sufficient for many purposes. It is only necessary to compare the source of light in question and the Hefner lamp for a certain part of the spectrum lying between λ_1 and λ_2. The curve L, Fig. 5, then enables us to find readily the energy in that portion of the spectrum by taking the difference between the corresponding ordinates.

It is obvious that the present researches have been of a preliminary nature. It is evident, for example, that the bands, which in the visible spectrum of the Hefner lamp are doubtless superimposed upon the continuous spectrum, produce deviations, small perhaps, from the law of radiation of a black body. I hope nevertheless that these results will have a certain importance in the study of feeble sources of light, of certain phenomena of luminescence and even in physiological optics.

NEW BOOKS.

Principles of Inorganic Chemistry. By HARRY C. JONES. New York, The Macmillan Company, 1903. Pp. xx + 521.

"The aim of this book is to add to the older generalizations those recently discovered, and to apply them to the phenomena of inorganic chemistry in such a way that they may form an integral part of the subject, and, at the same time, be intelligible to the student. Why should we continue to teach the chemistry of atoms to students on the ground of its being a little simpler, perhaps, than the chemistry of ions, or on any other ground if we know that it is not in accordance with the recently discovered facts? Or why should we continue to teach purely descriptive chemistry when the science of chemistry has outgrown this stage, and many of the most important relations have been accurately formulated in terms of the simpler mathematics?"

This quotation from the author's preface gives the key to his treatment of his subject. He has written a book on inorganic chemistry which is almost physical chemistry with its illustrations all chosen from the inorganic field. More than this, the book is written from the standpoint of the theory of electrolytic dissociation, whereas the phase rule, the law of mass action and other important generalizations are lightly treated. The author leaves us in no doubt about his devotion to the dissociation theory when he says in this preface, "We know to-day that nearly all inorganic reactions are reactions between ions; molecules and atoms as such having nothing to do with the reactions." He treats the whole subject furthermore as if these physical chemical generalizations were complete and final.

That these newer generalizations are valuable as aids in explaining many otherwise puzzling phenomena is generally admitted. But they may easily be overworked, and in the case of this book one has the feeling that the ionization theory has been. It is doubtful if Dr. Jones' book will fulfill its aim of being intelligible as far as the elementary student is concerned, and if it is intelligible to him the student will have an erroneous idea of the relation between the newer and the older generalizations.

The elements are grouped for study generally as they are in the Periodic Table, after the three elements, oxygen, hydrogen, chlorine — more or less typical, as the author calls them — have been taken up in some detail. The arrangement is generally good, though it is difficult to see just why neodymium and praseodymium should be considered

with vanadium, columbium, and tantalum, and not with lanthanum and the yttrium group. Among other omissions the author fails to give double salts more than very slight mention.

The book is well gotten up and the type is good, but the proof-reading was poor and the trivial errors are many.

Errors of fact like the following should not have crept into a text on inorganic chemistry. It is stated on page 254 that phosphorus oxychloride is formed by the action of water on the trichloride or on the pentachloride of phosphorus. This is not true as regards the action of water on trichloride. On page 305 we are told that bauxite is a titanium mineral, and on page 407 that it is a hydroxide of aluminum and iron; iron is nearly always, perhaps always, present, but bauxite is essentially a hydrated oxide of aluminum. On page 306 we are told that "thorium usually acts as a trivalent element forming salts of the type $ThCl_3$, $Th_2(SO_4)_3$, etc."; it is tetravalent in its salts. On page 315 sodium hydride is given the formula NaH, and on page 335 that for potassium hydride is similarly given KH. On page 334 metallic potassium is said to set metals such as aluminum free from their oxides; this is at least not true in the case of aluminum (see Goldschmidt and Vantin, J. Soc. Chem. Ind. *17*, 543). And why should the student be left with the impression that the "D" line is single, as is done on page 332; or that a single bright green line constitutes the barium spectrum, as on page 381? The melting point of rubidium is 38.5°, not 158°, as given on page 354.

The work is interesting reading to the advanced student, as it brings forcibly before him the general relations of the facts with which he is familiar. BENTON DALES.

An Elementary Treatise on the Mechanics of Machinery. By JOSEPH N. LE CONTE. New York, The Macmillan Company, 1902. Pp. 303.

Professor Le Conte has divided his subject into three parts: (1) An introduction, in which the general principles of kinematics are briefly demonstrated; (2) machinery of transmission; (3) the mechanics of the steam-engine, including both kinematics and dynamics.

To treat so broad a field within the compass of 300 small octavo pages results in a certain lack of proportion — which is the most serious fault to be found with the book. The author's trend of mind is toward rigid analytical demonstrations, leading him at times to apportion his available space in accordance with the mathematical difficulties of his topics rather than their importance in practice. However, when the result is so satisfactory as his treatment of skew wheels, for instance, one feels inclined to withdraw any objection. GUIDO H. MARX.

Vorlesungen ueber Experimentalphysik. By AUGUST KUNDT, edited by KARL SCHEEL. Pp. xxiv + 852. Braunschweig, Vieweg, 1903.

The lectures on experimental physics delivered by the late Professor Kundt during the last five years of his life, 1889–1894, in the Physical Lecture Room of the University of Berlin, will be remembered with great pleasure by all those who were fortunate enough hear them, on account of the unusual wealth and originality of the illustrations presented and of the clearness and simplicity of statement with which the demonstrations were accompanied. In the present volume these lectures, essentially in the original form of the manuscript notes of the author, are given to the public. The decision to retain as nearly as possible the condition and arrangement of the manuscript, making only such changes as were necessary to prepare the same for the press, was a wise one. We have as a result of this conservatism on the part of the editor a faithful record of the beginner's course in physics as it was given in the University of Berlin fourteen years ago; instructive from the standpoint of the development of the science, no less on account of what it does not than of what it does contain. The treatise comprises a year's work, eighty-seven lectures in the winter semester on mechanics, sound and heat and sixty-three in the summer semester on electricity and magnetism and light. There are neither footnotes nor references to the original sources.

E. L. N.

Alternate Current Transformer. By F. G. BAUM. Pp. i + 188. New York, McGraw Publishing Company, 1903.

After a brief outline of elementary principles, the writer discusses the graphical representation of pressure relations. As a basis, the secondary electromotive force is considered as being due to changing magnetism, rather than as due to changing primary current. The reviewer agrees with the writer that this treatment is to be preferred, since it more nearly represents the physical facts. The following four chapters, on regulation, efficiency, testing and design, constitute the useful portion of the book. The treatment in these chapters is good, being brief and to the point. The last third of the book, while containing some good material on transformer connections, is somewhat dilute, being interspersed with trade cuts of commercial types and descriptions of special appliances.

On the whole the book should form a satisfactory student manual; the treatment is sound and for the most part clear. The parts are well balanced and the author's individuality is shown throughout. The book is far from "mechanically as perfect as possible" as indicated in the preface.

F. BEDELL.

Modern Chemistry. By WILLIAM RAMSAY. Part I., Theoretical Chemistry, 120 pp. Part II., Systematic Chemistry, 203 pp. The

Temple Cyclopædic Primers. London, J. M. Dent and Company; New York, The Macmillan Company.

These little volumes present in brief and compact form the chief facts and theories of modern chemistry. The theoretical part is not a text-book on physical chemistry but the subject is presented from a physico-chemical standpoint with a breadth of view and a due observance of relative proportions that are admirable. In Part II. the same classification is followed as was used in the author's text-book, "A System of Inorganic Chemistry." The hydrides of all the elements are first considered, then the halides, the oxides and hydroxides, etc. This presents the subject from a point of view that differs from the customary one and that has many advantages. The style is clear, the books are eminently readable, and the amount of matter that Professor Ramsay has succeeded in compressing into their small compass is surprising.

THEODORE WHITTELSEY.

Lehrbuch der Kosmischen Physik. Von S. ARRHENIUS. Pp. viii + 1026. Leipzig, S. Hirzel, 1903.

In this treatise the term *cosmic physics* is taken in the broader sense of the word. It includes astrophysics and the physics of the earth's crust, to which two subjects, Part I. (containing 472 pages) is devoted, and meteorology, or the physics of the atmosphere, to which the remainder of the volume is given. The point of view and method of treatment throughout are those of the physicist rather than of the astronomer, the geologist or the meteorologist respectively.

As a work of reference Professor Arrhenius' treatise will be found of special value to the student of physics. It contains a wealth of interesting and important material which is excluded from the ordinary text-books and manuals of experimental physics for lack of space and which is omitted in the corresponding works on astronomy, geology and meteorology for the very reason that it is frequently of interest to the physicist rather than to the student of those sciences. E. L. N.

Lehrbuch der Physik. Von O. D. CHWOLSON. Erster Band. Übersetzt von T. H. PFLAUM. Pp. xx + 791. Braunschweig, Vieweg, 1902.

This German translation of Chwolson's compendious treatise renders a very valuable book available to a larger public. In bringing it out in its present form the translators and publishers are doing more than to merely add another to the numerous four-volume treatises on physics which have appeared in the German language; for Chwolson's treatment of his subject departs in many particulars from that of previous texts and his work contains many novel and interesting features.

The first volume contains an introductory section, in which the

nature of physical theories, hypotheses, laws and quantities is discussed. This is followed by a section devoted to mechanics; and under this head the author deals with motion, force and energy. Harmonic motion and wave systems are treated at considerable length and a number of topics are dealt with in this portion of the work which are frequently deferred until the subject of optics is reached. We find in this section on mechanics, for example, Huyghen's principle, the laws of diffraction, reflection and refraction and the principle of Doppler; from which the author returns to the consideration of universal gravitation, the potential theory and the pendulum. This section closes with a chapter on the dimensions of physical quantities.

One meets occasionally in this portion of the work conceptions of doubtful utility; as for example on page 89 where the gram is spoken of as a unit of force and equal to 981 dynes. On page 96 we find the field of force defined as a medium which possesses properties such that upon a body at a given place within the same a force acts which is proportional to the mass of the body.

The section upon mechanics is followed by one descriptive of instruments and methods of measurement and this contains a series of very useful chapters on dividing engines, and micrometers of various types; on planimeters and instruments for the measurement of angles; on the measurement of volume, of mass, and of the force of gravitation. The volume closes with a discussion of the properties of *gases*, *liquids* and *solids*, taken in the logical, but unusual, order given above.

References to the literature are gathered at the end of each chapter instead of being introduced in the form of footnotes and the citations are arranged in the order in which the various topics are taken up in the text. The usefulness of the treatise as a work of reference is further enhanced by an author's index and a fairly complete index of subjects.

Aside from its merits as a treatise on general physics, in which respect Professor Chwolson's work may be ranked with the standard manuals of the German authors, this work will have the advantage of bringing to the great body of working physicists to whom Russian is a closed book, a more complete knowledge of the contributions of Russia to our science. In addition to the memoirs and bulletins of the Academy of Sciences at St. Petersburg, frequent reference will be found to the journals of the Russian Physical Society, and the Moscow Association of Naturalists and to the programmes and publications of the various Russian universities.

E. L. N.

Mathematical Papers of the Late George Green. Edited by N. M. FERRERS. Facsimile reprint. Pp. x + 336. Paris, Librarie Scientifique A. Hermann. 1903.

The earlier editions of these important papers have so long been out of

print that the present new edition will be gladly welcomed. No additions or changes have been made; the edition is in fact a facsimile reprint of that of the year 1870. The editor's preface, in addition to a very brief account of the life of Green, gives a short sketch of each of his papers. The best known of these and the most important is probably the first — "An Essay on the Application of Mathematical Analysis to the Theories of Electricity and Magnetism." It will be remembered that this paper was originally published by subscription in 1828, before its author had begun his studies as an undergraduate at Cambridge. In it the term *potential* was first introduced and many of the properties of the potential function were investigated for the first time. In this paper also the celebrated "Green's Theorem" was first stated and proved, while its numerous applications in the theory of electricity were well illustrated.

The volume contains in all eleven papers. Among the other papers that have been of special importance in the development of Mathematical Physics, the following may be mentioned: The Determination of the Exterior and Interior Attractions of Ellipsoids of Variable Densities; The Motion of Waves in a Variable Canal of Small Depth and Width; The Laws of Reflection and Refraction of Light at the Common Surface of Two Non-Crystallized Media; On the Propagation of Light in Crystallized Media. E. M.

Handbook of Climatology. By JULIUS HANN. Translated by R. DEC. WARD. New York, The Macmillan Co., 1903. Pp. 429.

Hann's Handbuch der Klimatologie has long been a standard on the subject, but the fact of its being in German has prevented its use in America excepting by certain specialists. In order to make the book of use to students at Harvard University and elsewhere, Professor Ward has translated the first volume of the second edition. While the book is in the main a translation of Hann's original publication, Ward has added a number of new points from the standpoint of American climatology and both the translation and the insertions have received the approval of Dr. Hann.

The Centigrade scale and metric system are used in the volume, but at the end are a series of conversion tables. Professor Ward is to be commended for the sacrifice which he has made in rendering this valuable book more accessible to the English-speaking people and for the admirable way in which he has performed his task.

R. S. TARR.